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THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL:

EXHIBITING

A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

IN

MEDICINE, SURGERY, AND PHARMACY.

VOLUME FIFTEENTH.

1819.

EDINBURGH:
Printed by George Ramsay & Company,
FOR ARCHIBALD CONSTABLE AND COMPANY, EDINBURGH;
LONGMAN, HURST, REES, ORME, & BROWN, LONDON;
AND JOHN CUMMING, DUBLIN.

1819.

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Communications have been received from Drs Ballingall, Carrick, Marshall Hall, M'Keever, Richard Pearson, James Rutherford, Sinclair, and Whiting; and Messrs James Bell, G. Dempster, Fosbrook, Higgenbottom, Le Fevre, Sandwith, and Ritchie.

The following publications have been sent to us for review:

A Treatise on the Nature and Cure of Gout and Rheumatism. By Charles Scudamore, M. D. The third edition, revised and materially enlarged. 8vo. London, 1819.

A Comprehensive Treatise upon the Symptoms, Consequences, Nature, and Treatment of Venereal or Syphilitic Diseases. Translated from the seventh French edition of F. Swediaur, M. D. 2 vols. 8vo. London, 1819.

Clinical and Pathological Reports. By Samuel Black, M. D. 8vo. Newry, 1819.

Reports on the Diseases of London. By Thomas Bateman, M. D. 8vo. London, 1819.

Essays on Hydrocephalus Acutus, or Water in the Brain. By John Cheyne, M. D. 2d edition. 8vo. Dublin, 1819.

Researches on the Nature and Causes of Epilepsy, as connected with the Physiology of Animal Life and Muscular Motion. By John G. Mansford. 8vo. Bath, 1819.

An Inquiry into the Nature and Properties of the Blood, as existent in Health and Disease. By C. Turner Thackrah. 8vo. London, 1819.

Cases of Hydrophobia. By C. Pinckard, M. D. 8vo. London, 1819.

Report submitted to his Royal Highness the Commander-in-Chief, upon the subject of the Outpensioners of Chelsea Hospital that have been under Treatment for Diseases of the Eyes. 8vo. London, 1819.

A Letter to the Right Honourable Lord Viscount Palmerstone, upon the subect of the Ophthalmic Institution. Second edition, with an Additional Apendix. By John Vetch, M. D. 4to. London, 1819.

In the course of the present year will be published, an Account of the Rise, Progress, and Decline of the Fever lately epidemical in Ireland, by F. BARKER, M. D., and by J. CHEYNE, M. D. F. R. S. E.

Drs Barker and Cheyne, who were lately engaged, by command of his Excellency the Lord Lieutenant, in the inspection of the provinces of Munster and Leinster, have access to all the most important official documents relating to the prevention or suppression of the late epidemic Fever. Many of these they propose to introduce in the course of their work, together with the Reports of the Medical Inspectors on each of the provinces of this country.

Dublin, Aug. 1, 1819.

ERRATA.

Page 109, line 16, *for* amenhorrhoea, *read* amenorrhoea.
145, line 15, *for* latest, *read* latent.
150, line 21, *for* could be, *read* could not be.
157, line 30, *for* obtaned, *read* obtained.
294, line 16, *for* Mr Hay, *read* Mr Hey.
362, line 13, *for* or ss, *read* or ℥ss.
369, line 10, *for* ℥i, *read* ʒi.
429, line 29, *for* and the art, *read* and art.
437, line 35, *for* Jatro-hepatici, *read* Iatro-hepatici.
438, line 4 of the foot note, *for* inguinatâ, *read* inquinatâ.
454, line 34, *for* comprising, *read* composing.

EDINBURGH
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(Being the First Number of a New Series.)

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Communications have been received from Dr Barnes, Carlisle; Dr Breen, Dublin; Dr A. Crawford, junior, Winchester; Dr Howison, Edinburgh; Dr Kennedy, Dunning; Dr W. Ogilvie Porter, Bristol; Dr Thomson, Jamaica; Mr G. F. Edwards, Bath; Mr G. N. Hill, Chester; Mr S. G. Lawrance, Southampton; Mr M'Arthur, Paisley; Mr M'Cabe Mr M'Leod, North Uist; Mr Renton, Pennicuik; Mr Macartney Ross, Leith; Mr E. Welchman, junior, Kineton; Mr A. Word, Rochdale.

We return thanks to *a Subscriber and occasional Contributor* for his suggestions, which we shall endeavour to profit by.

We have just received (Dec. 17th) a letter from James Alexander, Esq. assistant surgeon 2d or R. N. B. Dragoons, who was hospital mate at the Depot, under Dr Vetch, containing the most decisive testimony in favour of that gentleman, in relation to the charges brought against him by Sir William Adams.

The following Publications have been *presented* to us for Review:

Clinical Report on Dropsies. By John Crampton, M. D. &c. pp. 136. 8vo. Dublin, 1818. From the Transactions of the College of Physicians of Ireland.

Remarks on the Pathology and Treatment of Typhus Fever. By Albert Henry Callannon, M. D. pp. 102. 8vo. Cork, 1817.

Practical Observations on the consequences of Insanity. By William Sanders Hallaran, M. D. Second edition, materially enlarged and amended. pp. 213. 8vo. Cork, 1818.

Practical Observations on the Extraction of the Placenta. By James Murdoch, M. D. &c. pp. 48. 8vo. Edinburgh.

A Manual of Practical Anatomy, for the use of Students engaged in Dissections. By Edward Stanley. pp. 439. 12mo. London, 1818.

Surgical Report, containing an Account of those Affections of the Penis which are generally considered as primary symptoms of Syphilis, with Modes of Treatment employed in the Richmond Surgical Hospital. pp. 45. 8vo. Dublin, 1818. From the Dublin Hospital Reports.

THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL.

1. JANUARY 1819.

PART I.

ORIGINAL COMMUNICATIONS.

I.

RESEARCHES ON THE PATHOLOGY OF THE BRAIN.

(Continued from Vol. XIV. p. 592.)

PART III.

ON PARALYSIS.

By JOHN ABERCROMBIE, M. D. Fellow of the Royal College of Surgeons of Edinburgh.

THE paralytic attack generally consists of loss of speech, with hemiplegia. In some cases the speech is not affected; in others loss of speech is the only symptom at the first invasion, and sometimes one limb only is affected, most commonly the arm. The attack is in some cases sudden,—in others it is preceded for some time by headach. It differs from the apoplectic attack in the absence of coma, the patient frequently appearing sensible of his situation, understanding what is said to him, and answering by signs. Many of these cases recover speedily and perfectly, the disease leaving no bad consequences behind it. (Cases 22 and 23.) In the progress of those which have not this favourable termination, the following varieties chiefly deserve our attention.

1. Many of them pass into apoplexy, perhaps in a few hours, the paralytic attack being only the prelude to the apoplectic.

2. Some of them, without shewing any tendency to apoplexy, do not recover. The patient, perhaps, improves so far as to be able to walk, dragging the paralytic limb by a great exertion, but after this makes no farther improvement; and after having continued long, perhaps years, in this condition, dies of a fresh attack, or of some other disease. (Case 20.)

3. In a third variety, the patient makes no improvement at all, is confined to bed in the most helpless state for several weeks, and then dies gradually exhausted, sometimes comatose for a day or two before death. (Case 19.)

In endeavouring to investigate the morbid conditions connected with these various forms of the disease, we meet with considerable diversity.

1. *In the cases which pass into apoplexy the same appearances are observed as in the apoplectic attack.*

1. *Extravasation of Blood.*—Some cases of extravasation are immediately apoplectic, others are at first paralytic. We know not the cause of this difference, for we frequently can detect no difference in the morbid appearances. In general, hemiplegia seems to be produced when the extravasation is on one side, either in one ventricle or on the surface, or in a new cavity in the substance of the brain. From the history of these cases, it is probable that coma is induced either by the extravasation passing to the other side, as from one ventricle into the other, or merely by increase of the quantity, though it may be confined to one side, the smaller quantity being connected with paralysis, the greater with apoplexy. A man mentioned by Morgagni had loss of speech and hemiplegia of the left side, and died in ten days; ʒii. of blood were found in the right lateral ventricle. Another with the very same morbid appearances had paralysis of the left arm only, and died on the 5th day. In a third, who had palsy of the left side, and died apoplectic in twelve hours, blood was found to have passed into all the ventricles. In some of these cases again, palsy appears on both sides before coma is induced, as in a case by the same writer, in which there was palsy of the whole left side, and of the right arm. Blood was found in all the ventricles, but it seemed to have come from the right, the substance of the brain there being lacerated. In other cases, complete palsy of both sides has been observed before apoplexy took place. Extravasation on the surface also occasions palsy of the opposite side, and, as the quantity increases, seems to induce coma. On the other hand, extravasation may take place in all these situations, inducing fatal apoplexy without having

induced paralysis. This is exemplified in several cases in this paper. In one, all the ventricles were full of blood, which, there was every reason to suppose, had come from the left; in another, all the ventricles had evidently been filled from a new cavity in the right hemisphere, and in a third, from a similar cavity, blood had spread extensively on the surface of the right hemisphere But in none of these cases was there any paralytic symptom, though in all of them there was a considerable interval betwixt the attack and the occurrence of coma. It appears, therefore, that paralysis is not necessarily produced by extravasation of blood, though confined to one side, taking place slowly, so as to give opportunity to observe the progress of the symptoms, and in such quantity as at last to produce fatal apoplexy.

2. In many of these cases we only find on dissection serous effusion, often in small quantity. A man mentioned by Morgagni had palsy of the right arm, and died apoplectic in two days. On dissection no morbid appearance was observed except serous effusion, both in the ventricles and on the surface of the brain. Another had loss of speech and palsy of the left side, and died comatose at the end of a month. Considerable effusion was found on the surface of the brain, but very little in the ventricles. A third had loss of speech and palsy of the right side, then became comatose, and died in five days. The ventricles contained about ℥ij. of fluid; there was also a good deal on the surface of the brain, which appeared to be most abundant on the right side. I have formerly given my reasons for believing that serous effusion, in apoplectic cases, is not a primary affection, but a termination of the state which I have named simple apoplexy. I have also described cases in which such effusion existed without paralysis, and in the cases in which they existed together, the effusion was distributed equally over the brain, except in one case now quoted from Morgagni, in which it was most abundant on one side, and that was the same side with the disease. From all these considerations we must conclude, that, in the cases to which I now refer, the effusion was not the cause of the paralysis, but that it was the effect or the termination of a certain state of the circulation inthe brain with which the paralysis had been connected from the first invasion of the disease. This state of the vessels, affecting the whole brain equally, constitutes simple apoplexy. I see no objection to the supposition that an analogous disorder of the circulation may affect part of the brain, deranging the functions of that part only, so as to produce paralysis of certain muscles. The whole phenomena of palsy bear evidence that many cases of it depend upon some such inorganic, or what I may term

moveable cause. We see hemiplegia take place in the highest degree, and yet disappear completely in a few days. We see it disappear more gradually, so that the parts are perfectly recovered in a few weeks. We see it exist for many weeks or months, without any improvement, and then, from some change which eludes our observation, take a turn for the better, and very suddenly disappear. These circumstances seem to give considerable probability to the conjecture which I have hazarded; their reference to the treatment of paralysis I shall have occasion to allude to afterwards.

II. *The old cases of palsy, in which the patient has continued for years without improvement, though otherwise in tolerable health, and has at last died of some other disease*, present a most interesting subject for investigation, which I can at present allude to but very imperfectly. I do not here refer to those cases which have come on gradually, and are connected with organic disease, but to those which have either supervened on an apoplectic attack, or have come on suddenly in the proper paralytic attack. Now, these cases are often cut off by a fresh paralytic or apoplectic attack, and then we cannot say with certainty which of the morbid appearances were connected with the old, and which with the recent disease. Hence the importance of those cases in which death at last takes place from some other disease; a faithful record of such cases would tend to throw much light on the pathology of paralysis. I have described a remarkable example of this kind, (Case 20,) in which the disease continued without relapse and without improvement for fifteen years, and yet nothing could be detected but serous effusion in the brain, with a slight appearance of disease in the longitudinal sinus. A man, mentioned by Morgagni, died of pneumonia, after having been long affected with paralysis of the right side, accompanied by loss of feeling; the disease had remained after an old apoplectic attack. No morbid appearance could be detected in the brain, except some serous effusion on the surface, and a still smaller quantity in the ventricles. A woman, mentioned by the same writer, had hemiplegia for three years, was confined to bed, and died of gangrene of the nates:—there was considerable effusion on the surface of the brain, very little in the ventricles, and no other morbid appearance. In cases of this kind, we are in the habit of believing that some deep-seated and irreparable injury has been done to the brain. Perhaps this opinion has been received without sufficient examination. The cases which I have mentioned were of the most favourable kind for ascertaining the point, yet no such injury could be detected. This opens a wide and most interesting field of inquiry which

I am not prepared to enter upon at present; but I think there is much reason to believe, that many of these paralytic affections are connected with some disordered state of the circulation in the brain, or with a state of disease which is not organic, and which we are not entitled to consider as hopeless.

Among the causes of these old paralytic affections, have generally been reckoned cavities in the substance of the brain, containing the remains of former extravasations of blood; and some writers have contended, that extravasated blood may lodge in this manner for a long time in the brain, not only without being fatal, but even without producing any urgent symptom, after the removal of the original apoplectic attack. I have some doubts whether this opinion rests on any good foundation. The case in Morgagni which is often referred to in support of it, was that of an old woman who died apoplectic in the hospital of Pavia. On one side of the brain was found a large cavity containing extravasated blood of a recent appearance, and on the other side a small cavity containing a dark-coloured glutinous matter, which he supposed to be the remains of blood extravasated in a former attack. It is added, however, that no account could be obtained of the previous history of the patient, nor even a correct account of the fatal attack itself, consequently there is no evidence of a former attack.[*] Now, in case 17th of this paper, the same appearances occurred as in the abovementioned case of Morgagni. In this case, however, I know that there never had been any attack previous to the fatal one, consequently, it is reasonable to conclude, that the two extravasations had taken place at different periods of the same attack; and I must believe that this might have occurred in Morgagni's case, except I see some reason to believe the contrary. I do not think the other cases mentioned by Morgagni on this point are more satisfactory. In the two cases, Epist. LX. § 2 and 6, there were distinct extravasations; but there is no evidence that these had taken place in distinct attacks; for we have every reason to believe, that several different extravasations may take place in the same attack, as in a case by Dr Cheyne, formerly referred to, in which it had taken place in each of the corpora striata, and in the third and fourth ventricles. Wepfer's case, quoted by Morgagni, proves nothing, for he only supposes, from the direction of the wound, that extravasation must have taken place; and the case communicated to Morgagni by Plancus was evidently of a different nature, for he says expressly it was an abscess. It seems to have been an old disease of the brain, in which paralysis was connected with chronic induration, and was fatal by suppuration, a disease of which I have given several examples in a former paper. Some other cavities which have been supposed to be the remains of apoplectic attacks, I think have evidently been the result of chronic inflammation, and not of apoplexy. The same observations I think apply to Mr Abernethy's cases, * and, upon the whole, I consider these important questions as still undecided ; Whether extravasated blood may lodge in the brain without being soon fatal ? What change the blood undergoes in such a cavity ? and Whether such extravasation ever exists as the cause of long continued paralysis ?

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[*] De Causis et Sedibus Morborum. Epist. III. § 6.

In a case by Rochoux, † there is reason to believe that a cavity of this kind, containing coagulated blood, had continued three months and a half, and this is the longest period that I have been able to find in any case, the history of which I can consider as satisfactory. In this case, the patient was completely paralytic on the right side, and the intellectual faculties were very much impaired. She died of coma, coming on gradually, without a fresh apoplectic attack ; and an irregular cavity was found in the left corpus striatum, an inch in diameter, containing blood which was "fibrous, filamentous," and of the colour of "terre de Sarguemine." Another case by the same writer, which was of longer continuance, was evidently of a different nature, for the cavity contained purulent matter.

III. *An important circumstance in the history of paralytic affections is, that many of them are connected with a state of the brain which is not apoplectic, but inflammatory.* I do not here allude to paralytic affections occurring at an advanced period of such diseases, and connected with suppuration, but to paralysis taking place suddenly, so as to bear a close resemblance to the proper paralytic or apoplectic attack. The first attack in these cases is frequently accompanied by convulsion, and the farther progress of them is sometimes that which I have mentioned as the third form of the paralytic affections. In other cases, after a certain period, the disease puts on the proper characters of inflammatory action. In some cases, partial paralytic affections are the first symptoms, while no symptom appears which distinguishes them from ordinary paralytic attacks, or points out their inflammatory origin. A young man, for the history of whose case I am indebted to an eminent practitioner, Mr Clarkson of Selkirk, after bathing in the Tweed, lay down on the bank and fell asleep without his hat, in a very hot day, in June 1818. When he awoke, he had lost his speech, but walked home, and appeared to be otherwise in good health. He recovered his speech imperfectly on the following day, and afterwards lost it, and partially recovered it, several times during the four or five succeeding days. During this period he was observed to be dull and forgetful. Afterwards he had dilated pupil, squinting, and double vision, with a dull uneasiness in the back part of his head, without acute pain. His pulse varied from 60 to 86. Notwithstanding the most judicious and active practice, he sunk gradually into coma, and died in 24 days. On dissection, a considerable part of the brain was found in a state of suppuration; the remainder exhibited marks of inflammatory action, and there was effusion in the ventricles. This affection may be more rapidly fatal, so as to shew the disease in its first or inflammatory stage, and with a still closer resemblance to apoplectic paralysis. A man mentioned by Morgagni, had severe headach, speedily followed by hemiplegia of the right side, and then palsy of all the lower parts; he died in a few days. On dissection, the whole medullary substance of the right hemisphere was found of a dark brown colour, with considerable effusion in the ventricles. I think there is every reason to believe that this was an example of the same disease, which, at a more advanced period, produced the appearances described in case 19th of this paper, or that they were both of an inflammatory nature, the first proving fatal in the early stage, the second by suppuration, at an advanced period. I have, in a former paper, given reason to suppose, that chronic inflammation may go on for a considerable time before it comes to suppuration, and I have described a case, which terminated by suppuration, after the patient had been in the most helpless state of hemiplegia for more than two months.* Numerous examples are on record which illustrate this modification of the disease. A man mentioned by Morgagni, was suddenly attacked with hemiplegia of the right side, accompanied by delirium and convulsion, and died in a few days. The only morbid appearance was, that the left lateral ventricle was full of purulent matter. A woman, aged 59, was suddenly attacked with loss of speech, and paralysis of the right side and right eyelid, followed by coma and death after several days. An abscess, the size of a large walnut, was found in the substance of the left hemisphere, in which soft corrupted cerebral substance was mixed with some bloody fluid; there was also effusion in the ventricles. In another case, there was sudden loss of speech and death in two days, and the anterior part of both hemispheres of the brain was covered with purulent fluid. There was also effusion under the arachnoid membrane. A man, aged 60, had been ill for some time with violent tormina and diarrhœa, when he was suddenly seized with hemiplegia of the right side, and loss of speech, and died on the fourth day, with his other faculties entire. The left corpus striatum was found so eroded, as to be nearly separated from the neighbouring part of the brain. The surface of the brain on the left side was also eroded in two places, and there was effusion in the ventricles. In one of the cases now mentioned, it will be remarked that the palsy was on the same side with the disease in the brain. A similar case is mentioned by Dr Coindet. A man, aged 60, after being for some time affected with loss of memory, was seized with severe headach, paralysis of the left side, and convulsion of the right. He died in four days. The whole left hemisphere was found reduced to a state of remarkable softness; there was considerable effusion in the right lateral ventricle, and not a drop in the left. * A similar case, with paralysis on the same side with the disease, is related by Bonetus. † A similar disease, with palsy on the opposite side, occurred in a boy mentioned by Sauvages. He had palsy of the right side, at first accompanied by profound sleep, afterwards by incoherent talking, and he died in four days. The brain and cerebellum, on the left side, were found "putrid, corrupted, and sphacelous." ‡ In a case of hemiplegia of the right side, fatal in sixteen days, mentioned by Dr Home, § the only morbid appearance was a considerable quantity of bloody purulent fluid, on the surface of the medulla oblongata on the left side, and the same appearance about the medulla spinalis.

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* Abernethy on Injuries of the Head.

† Rochoux, Recherches sur l'Apoplexie, p. 95.

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* On Chronic Inflammation of the Brain, Case X.

A woman mentioned by Wepfer had very much an apoplectic appearance. She was carrying a load upon her head in a hot day, when she felt as if something had given way in her head; and under a similar exertion, some days after, she expe-

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* Coindet, Mémoire sur l'Hydrencephale, page 47.

† Boneti, Sepulchretum Anatomicum, Lib. I. sect. 3. obs. 34.

‡ Sauvages, Nosolog. Methodica, Vol. I. p. 834.

§ Home's Clinical Experiments, p. 252.—See also a case by Dr Duncan jun. Reports, &c. No. xxv.

rienced the same feeling to such a degree that she nearly sunk under it. From that time she could not retain her urine, but had no other symptom, until several months after, when she was suddenly seized with hemiplegia of the left side; her speech was indistinct, and her mouth distorted. She had violent pain in the back of the head, and in the right eye, and was delirious for fourteen days. She then became sensible, and began to improve; her speech became distinct, and after eight weeks she had slight motion of the paralytic limbs. The motion improved gradually, and after some time she could walk with the assistance of a stick, but still complained of headach. About eight months after the former attack, in rising out of bed one morning, she felt a return of the paralysis of the left side, and a severe pain in the sole of the right foot: she had also vertigo and severe headach, referred chiefly to the right side of the head. After continuing several months without any farther change, she became dropsical, with cough and dyspnœa; severe pain continued in the right foot, and she died, gradually exhausted, about seven months from the last attack of hemiplegia, having retained her sight, hearing, and mental faculties to the last. On dissection, much effusion was found, both on the surface of the brain, and in the ventricles. An abscess, the size of an egg, surrounded by a very firm sac, was found bordering on the right ventricle. It contained a turbid fluid, and the cerebral substance near it was eroded and ulcerated. * The case of another woman mentioned by the same writer, illustrates a different modification of the disease, and shews how long it may go on without being fatal. After suffering for some time from severe headach, she fell down apoplectic, and lay in that state for three days. She then recovered, with palsy of the right side; and after some time, this improved considerably, so that she could walk with assistance. She then became maniacal, often escaped and wandered in the woods, and several times attempted to destroy herself; the right side remained much weaker than the other. After continuing in this state for three years, she died of pneumonia. On dissection, the pia mater was found thickened, and very vascular; there was considerable effusion both in the ventricles and on the surface of the brain. Both lateral ventricles and the third ventricle were covered on their inner surface by a thick mucus of a crocus colour, and there was the same appearance on the choroid plexus. There were

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* Wepfer, Historia Apoplecticorum, p. 558.

many hydatids on the choroid plexus on both sides, as large as peas, and one on the left side as large as a filbert. These examples will serve to illustrate this modification of paralysis, which is not connected with an apoplectic, but an inflammatory action in the brain. It appears that there is no uniformity of the symptoms, nor any precise mark by which it can be distinguished from the ordinary apoplectic paralysis. Many of the cases are accompanied by convulsion, but this does not occur in all; the convulsion sometimes appears on the same side with the paralysis, the convulsion attacking first and leaving the part paralytic; and sometimes they appear together, the convulsion on the one side, and the paralysis on the other. Many of them, again, are remarkable from not passing into coma, the patient remaining entire in his intellect to the last, or until a very short period before death; others, however, pass into coma at an early period. In some cases the pulse is frequent, but in others it resembles the pulse of apoplexy.

I think there is reason to suppose that some of these cases of inflammatory paralysis are distinguished from the apoplectic paralysis, by the attack in the former being less rapid. A man, aged about 35, (a shoemaker,) whom I saw very lately, under the care of Dr Tweedie, and whose case I believe to be of this kind, had pain extending from the left side of his forehead along the upper part of the head, and a remarkable loss of feeling in his right cheek. This loss of feeling extended gradually down the right side of his neck, and to his arm; the arm became weak, and the weakness increased gradually. The leg of the same side then became affected in the same manner; but the progress of the complaint was so gradual that he continued his work for a week after the attack; his leg and arm were becoming gradually more and more paralytic, but it was not till the end of the week that the paralysis had increased to such a degree as rendered him unable for his usual labour. This occurred about a week ago, and since that time the complaint has been nearly stationary. He can walk with a dragging and imperfect motion of the right leg, but has very little use of his arm; he has still pain along the upper part of the head; the numbness of his cheek is gone, and his speech has never been affected. This course of symptoms constitutes a case remarkably different from the attack of the apoplectic hemiplegia, which is generally sudden and complete. Whether the difference depends upon such a difference in the cause as I here suppose, I do not determine, but rather, at present, propose it as an interesting subject for observation. I think it is rendered probable by many of the facts which I have mentioned, both in this paper, and in my former paper on Chronic Inflammation of the Brain. I do not, however, mean to say that every case of inflammatory paralysis is distinguished by this particular progress of the symptoms. On the contrary, in some cases that are ascertained to be of this nature, the attack seems to be as rapid as in the apoplectic paralysis; and we naturally expect such differences in the inflammatory cases, depending probably on the extent of the disease in the brain, which in one case may affect at once a large portion of it, and in another may begin in a very small portion, and extend gradually. It appears, however, from several of the cases which I have mentioned, that disease of this kind, of no great extent, may be connected with extensive paralysis. I have already alluded to those cases in which the paralytic attack is preceded or accompanied by convulsion. These will probably be found to be of the inflammatory kind, especially when the convulsion has been confined to one side of the body, or to one limb, paralysis of the part immediately succeeding it. A remarkable example of this has been described in my paper on Chronic Inflammation of the Brain. (Case 7.)

A most important circumstance in the history of the inflammatory paralysis is, that all the symptoms may take place while the disease in the brain is in the state of simple inflammation, and that it may not have advanced beyond that state while they go through the usual course, and terminate in fatal apoplexy. I lately saw a young woman, who, after being for some time affected with symptoms indicating a tendency to disease in the brain, was found one morning to have lost her speech. She then gradually sunk into coma, with paralysis of the right side, and died on the ninth day. The only morbid appearance was a part of the brain, the size of a large walnut, in a state of high inflammation; it was at the upper and inner part of the left hemisphere, in contact with the longitudinal sinus. There was no effusion, and no other morbid appearance, except adhesion of the membranes to the inflamed portion by coagulable lymph, which was deposited between them. A woman, aged 30, whose case is related by Dr Treutler,* had been ill for two months with dropsy, which had followed intermittent fever, and was connected with disease in the spleen. In the third month of her illness, she complained of a feeling of weight in the occiput, towards the right side, with dimness of sight and a great propensity to sleep. Her hearing became daily more and more obtuse, her speech was very indistinct, and her memory was lost. She at last seemed to lose the power of every voluntary muscle, so that she could neither move her legs nor arms, nor raise her head. Finally, she had convulsions and apoplectic attacks, and died suddenly about the end of the third month of her illness; that is, less than a month from the commencement of these symptoms in the head. In the posterior lobe of the right hemisphere of the brain, behind the lateral ventricle, a portion, the size of a large walnut, ("fructus regiæ juglandis,") was in a state of high inflammation; the membranes adhered to the surface of the brain in several places; where that did not occur, there was serous effusion under the arachnoid membrane. There was no fluid in the ventricles; there were hydatids in the choroid plexus, which were most numerous on the right side. The spleen was much enlarged, and extravasated blood, to the amount of several pounds, was found in the abdomen; it was partly contained in the cavity of the omentum, partly between the laminæ of the mesocolon, and partly under the peritonæal coat of the descending colon. In a former paper, I have referred to a remarkable case by Mr Howship, * in which inflammation appeared to have spread along the membranes of the brain from one place to another, and at last to the membranes of the spinal cord, producing, in its course, a succession of urgent symptoms, and leaving distinct traces of its progress in extensive deposition of coagulable lymph. At an early period of the complaint hemiplegia occurred, which went off after a short time, though the disease advanced progressively to a fatal termination. It is therefore not unreasonable to conclude, that the paralysis existed in connection with active inflammation of the membranes; for long before death, when the inflammation was found to have terminated by extensive deposition of coagulable lymph, the paralysis had disappeared.

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* Treutler, Auctarium ad Helminthologiam Humani Corporis, p. 1.

The terminations of the inflammatory paralysis, when its progress is not arrested at an early period, are principally three. (1.) It may be fatal, as we have seen in the inflammatory stage, with all the symptoms of perfect apoplexy. (2.) It may be fatal by suppuration, and this may be either in the form of the encysted abscess, or of that extensive undefined suppuration which has been called sphacelismus cerebri. † (3.) The inflam-

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* Howship's Observations in Surgery and Morbid Anatomy, p. 74.

† In my paper on Chronic Inflammation of the Brain, I have stated that I had not seen the undefined suppuration (or sphacelismus) accompanied either by convulsion or paralysis. From several cases, however, which are mentioned in

mation may subside, leaving induration of a part of the brain, and thus producing permanent paralysis. This state of disease may continue a long time without being fatal. I have endeavoured to trace its progress when treating of chronic inflammation of the brain. It is chiefly when the patient dies of some other disease that we find it in the state of simple induration; when the disease itself is fatal, it is generally by the indurated part passing into suppuration. A gentleman whom I saw lately along with Mr William Brown, had been for four years affected with paralysis of the right side, and indistinctness of speech. Without any return of symptoms in the head, he died gradually exhausted by dyspnœa and general dropsy, connected with disease of the heart. In the anterior part of the left hemisphere, we found a portion of the brain, the size of a large walnut, very much changed in its appearance, being of a brownish yellow colour. This portion was much harder than the healthy cerebral substance, except at its lower part, where it was soft, approaching to suppuration. Induration of a part of the brain, however, does not always produce paralysis, it sometimes occasions convulsion. Several examples of this I have mentioned in my former paper, in one of which the convulsion was confined to the leg and arm of the right side, the induration being in the left hemisphere. On this interesting subject I shall only add a remarkable case by Mr Hill, * which illustrates several important points in the history of the inflammatory paralysis. A girl, aged 19, was first seized with numbness of the left hand, which extended gradually along the arm, and was accompanied by headach and vomiting. After two months the whole left side had become paralytic, and at the end of another month a small tumour, like a pea, appeared by the side of the bregma, which being repeatedly opened with a lancet, discharged matter with some relief. After seven months more, Mr Hill found a small hole in the right parietal bone, about a quarter of an inch in diameter, which was plugged up by a firm substance from within. The trephine being applied at this place, an abscess was discovered within the cranium, which discharged about ℥iv. of matter, and a small excrescence like a wart, arising from the dura mater, which had plugged up the orifice in the bone. There was considerable relief after the discharge, but protrusion of the brain took place; and she died,

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this paper, it appears that it may be accompanied by either or by both of these affections.

[*] Hill's Cases in Surgery, p. 150.

gradually exhausted, two months after the operation, having retained her faculties until two days before her death. On dissection, much effusion was found in the ventricles, and destruction of the brain by suppuration for about two inches round the opening in the skull.

In a former paper I have referred to many cases in which remarkable symptoms, both paralytic and convulsive, were connected with inflammatory action affecting various parts of the brain; some in which they were connected with inflammation and thickening of the membranes, and others in which very urgent symptoms of this kind were connected with a singular disease of the pericranium. In one remarkable case, in which the right arm was paralytic and withered, there was a small tumour on the left parietal bone with caries of the bone under it, and a perfect cure was obtained by the application of the trephine.

IV.—*Many paralytic affections depend upon diseases of the spinal marrow.* For these I refer to a former paper on that subject.

V.—*A paralytic state is known to follow severe rheumatism.* It may also be induced by long continued cold, without rheumatism having taken place. A man mentioned by Dr Clarke * became paralytic in both legs, and partially in the arms, in consequence of being much benumbed with cold in travelling on the top of a coach. He derived benefit from mercury and warm-bath, and was nearly recovered in eight or ten months.

Dr Powell has described three cases of paralysis of one side of the face, producing great twisting of the mouth, and in one of them inability to shut the eyelid. In all the cases the affection came on immediately after exposure to cold by a cold wind blowing upon the side of the face which was affected. It was not accompanied by any other symptom, and they all recovered; two of them were well in eight or ten days; the third, a child, was not quite free of the complaint for three months. They seemed to derive benefit from sudorifics and the application of steam. †

VI.—*There is a singular modification of paralysis which seems to be connected with the state of the circulation in the affected part.* A lady mentioned by Dr Storer ‡ was recovering from a pneumonic attack, when one morning, after a restless night, she was suddenly seized with an acute pain in the left shoulder, extending to the arm, and at the same time the whole left side became paralytic. The leg retained its feeling, but the hand and the foot were insensible to the prick of a needle. The parts were cold, and all the arteries in them were without pulsation. After a few hours the pain shifted to the leg and foot, and was there so severe as to occasion screaming. On the right side of the body the pulse was of good strength, and a little frequent. She had some obscure pain in the forehead, which was removed by bleeding with leeches. The pain of the leg and foot abated after twelve hours, and she had then no complaint except the paralysis. For several days she seemed to be improving a little in the motion of the parts, but they continued cold and without pulse. On the 5th day she had an uneasy feeling at the epigastrium, with sense of suffocation; her breathing became short and hurried, and she died in the night. The body was not examined. A gentleman, mentioned in the same paper, was seized with paralysis of the right arm as he sat at breakfast, having been previously in perfect health. He complained of no pain, but the arm was pale, and in every part of it without pulse: in the left arm the pulse was natural. After four hours he became faint, with quick and laborious breathing, and frequent pulse, and in two hours more died. The body was not examined. In the same Journal Dr Wells has described the case of a gentleman, subject to cough and dyspnœa, who awoke one morning with a severe pain in the left arm; in the afternoon it became benumbed and paralytic. The pain then ceased, and the arm was found to be without pulse. He continued in this state for two days, without any other complaint; and on the third day died suddenly, as he got up to go to stool. The paralytic arm only was examined after death, and in it no morbid appearance could be detected.

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* Edinburgh Medical Journal, Vol. IV. p. 266.

† Medical Transactions of the College of Physicians of London, Vol. V.

‡ Transactions of a Society for the Improvement of Medical and Surgical Knowledge, Vol. III. p. 448.

These singular affections were probably connected with disease of the heart or great arteries. Several years ago I saw a case which may throw some light upon them, though the symptoms were not exactly similar. A woman, aged 73, having been previously in her usual health, was suddenly seized with violent pain of the whole right arm, accompanied by palpitation of the heart, inclination to vomit, and a pain extending across the thorax, from the breast to the back. The pulse of the affected arm was extremely weak, in the other arm it was 120, and strong, but irregular. After a day or two the pain in the right arm ceased, leaving the arm without pulse and weak, but not completely paralytic. After ten days the right thigh and leg were affected in the same manner, and went through the same course; then the left arm in five days more; and ten days after this the left thigh and leg. She was then confined to bed in a state of great weakness, and no pulsation could be felt in any artery except the carotids, and a little in the right humeral. In the carotid it was strong and frequent. The radial artery was felt like a firm cord under the finger, as if permanently distended with blood. She still had pain in the region of the heart, which at times became very severe; it impeded respiration, and prevented her from lying on the left side. She lived a month in this state; the dyspnœa and palpitation became gradually more and more severe, and she died gradually exhausted, two months after the commencement of the disease. Some days before death slight pulsation was perceived in the arteries of the left arm, and in the right it was more distinct than formerly. On dissection much fluid was found in the pericardium; and in the right cavity of the pleura. The heart was flaccid, and none of its cavities contained any blood. In the right sinus venosus there were two firm fleshy tumours or polypi; one the size of a pigeon's egg, attached to the side of the sinus by a slender pedicle, the other smaller, and more extensively attached. The whole arterial system was extensively ossified; in some places the diameter of the artery was considerably diminished by the ossification, and several of the great arteries were obstructed by firm coagula at particular points. This was most remarkable in the right common iliac, which was filled through nearly the whole extent of the common trunk, by a dark-coloured coagulum which was very firm, elastic, and dry. The left subclavian was also much diseased, and considerably contracted, and the aorta near the bifurcation was for about two inches almost entirely ossified. There was considerable effusion in the ventricles of the brain. The sinuses of the dura mater were remarkably empty.

This kind of disease may exist in a more limited degree, affecting only the vessels of a particular part of the body. In this case it is apt to terminate by extensive gangrene. The limb is first affected with violent pain; this continues for a day or two; when it ceases the arteries of the part affected are found to be without pulse, and after some time it falls into gangrene. The gangrene which attacks the feet of old people is probably connected with disease of this kind in the extreme arteries. In a remarkable case of it mentioned by Mr Naish,* the gangrene began in the toes in the usual manner, and extended gradually, until in about a month it had reached the middle of the leg; the leg was then amputated four inches above the mortified part. During the operation two or three ounces of blood issued from the muscular parts, but on slackening the tourniquet not a drop of blood flowed, and the extremity of the artery was found hard and callous. The patient died on the fourth day. On examining the amputated limb, the arteries were found through their whole course extensively ossified. The trunk of the artery at the place of amputation was ossified in two-thirds of its circumference. About a quarter of an inch farther down it was entirely ossified, and its diameter so contracted that it would only admit a hog's bristle. Tracing the branches downwards, they were found in some places entirely ossified, in others free from ossification, and in others ossified on one side and membranous on the other. The disease was traced into some of the smallest branches in the foot. A gentleman, aged 67, whose case is described by Mr Cowper, * had lost the use of his lower extremities for nearly twenty years, and during that time had been so distressed with convulsive motions of the affected limbs, that sometimes he was not free from them for a quarter of an hour together, whether sleeping or waking. At length the toes of the left foot began to mortify, and the disease spread gradually, until unhealthy ulceration had extended above the ankle, laying bare one of the metatarsal bones. While this disease was going on, the convulsive motions of the left leg had been stronger than ever. The leg was amputated five inches below the knee, and it was remarked that there was very little bleeding from the arteries. On examining the amputated limb, the arteries were found ossified in the same manner as in Mr Naish's case. Six weeks after the operation gangrene attacked the other foot, and was fatal. The same writer describes a similar disease in the arm of a young woman, in whom also amputation was employed without success. The humeral artery was found extensively ossified, and so thickened in its coats that the cavity was contracted to less than a third of its natural diameter. It is remarkable that gangrene had not taken place in the case which I have described, as both the lower extremities had been without pulsation in the arteries for a month, and from the appearance of the right common iliac, it had evidently been for a considerable time completely impervious. In limbs that have been destroyed by extensive ulceration, remarkable dimi-

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* Philosophical Transactions, Vol. XXXI. p. 226.

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* Philosophical Transactions, Vol. XXIII. p. 1194.

nution of the arteries has been met with. Two cases of this kind are described by Mr Grainger,* which were saved by amputation, after such an extent of disease had taken place in one of them, that the leg as high as the knee was "denuded of all vessels, muscles, and ligaments, as are the bones collected in a charnel-house." In one of them he could find no artery that required to be tied, and in the other the trunk of the femoral artery could have been stopped with a pin.

There are singular facts upon record which seem to indicate peculiarities of the circulation in particular parts of the body, probably originating in the relation betwixt the vascular and nervous systems. I know a gentleman who, when heated by exercise, perspires over one half of his body and not on the other, the line being drawn with great precision from the forehead along the centre of the nose, and so downwards. When he is very much heated the other side perspires also; but this only occurs from great exertions, or great warmth; the singular perspiration of one side which I have mentioned is a matter of almost daily observation. A child mentioned by Dr Falconer became pale and emaciated on the whole left side of the body, without any evident diminution of muscular power, the right side remaining healthy. She recovered by the use of warm pumping. †

VII.—There are various other cases of paralysis which are foreign to the subject of this paper. Among these may be reckoned the palsy produced by lead and other poisons, by injuries of particular nerves, or tumours pressing upon nerves. Muscles also become paralytic from various injuries, such as overdistention. I have several times seen the deltoid remain paralytic after luxation of the humerus, and recover its action gradually, perhaps after several months.

In regard to the paralytic state in general, there are several important circumstances which may be here referred to very shortly. In some cases of palsy there is loss of motion without loss of feeling; in others the feeling is lost also. Cases are also related, in which loss of feeling took place without loss of motion. Several examples of this are described in the Memoirs of the Royal Academy of Sciences. The most remarkable is the case of a soldier, a very strong man, and able for all his duties, who had so completely lost the feeling of his right leg and arm, that he allowed the parts to be cut, or red hot iron applied to them, without complaining of any pain. A gentleman mentioned in the same paper had the same peculiarity in his right arm.[*] In a case which is related in the Ephemerides Naturæ Curiosorum, there was loss of motion on the one side, and loss of feeling, without any diminution of motion, on the other.[†] In cases in which there has been loss both of motion and feeling, we frequently observe recovery of feeling without recovery of motion. Berdotus, on the other hand, describes a case in which there was recovery of motion without any recovery of feeling.[‡] A similar case is related by Burserius.[§] Increased acuteness of feeling in paralytic limbs has also been observed; and in a former paper I have referred to a case in which, connected with a disease in the brain, there was such an increased sensibility of the arm that the least breath of cold air excited convulsions. Dr Falconer[||] mentions a gentleman who, after a paralytic attack, had such a morbid state of sensation, that cold bodies felt to him as if they were intensely hot. When he first put on his shoes he felt them very hot, and as they gradually acquired the warmth of his feet, they appeared to him to cool. Paralytic parts sometimes become intensely painful; this occurred in case 19th; and in a former paper I have mentioned a case in which recovery from palsy was accompanied by such pain that the limb remained useless. When paralytic limbs are recovering, the recovery sometimes begins at the extreme parts of the limb, as the fingers and toes, and extends gradually upwards; and sometimes it begins in the parts next the body, and extends gradually to the extreme parts. Paralytic affections often begin by affecting a very small part of the body, as one arm, or the hand only, or sometimes one finger; the muscles of the tongue, of one side of the face, of the eyelids. We know not the cause of these varieties. Paraplegia generally depends upon disease in the spine. It is sometimes, however, produced by affections of the head, as indurations of the cerebellum, or tumours about the medulla oblongata. It rarely occurs in connection with the apoplectic state, but there are examples of it. One is mentioned by Boerhaave in which it preceded apoplexy, and extravasated blood was found under the cerebellum, and about the top of the spine. In another, by Dr Cheyne, there was extravasation in the third and fourth ventricles.

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* Grainger's Medical and Surgical Remarks, p. 182.

† Memoirs of the Medical Society of London, Vol. II. p. 208.

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[*] Mem. de l'Acad. Royale des Sciences, an. 1743.

[†] Eph. Naturæ Curios. Cent. II. Obs. 196.

[‡] Act. Helvet. T. VI. p. 191.

[§] Institut. Medicin. Pract. Vol. III. p. 76.

[||] Mem. of the Med. Society of London, Vol. II. p. 206.

An interesting part of the history of paralytic affections refers to those cases in which the paralysis is confined to one muscle or one set of muscles, and remains in that state for a long time without improvement, and without going farther. I saw a man lately whose mouth is so twisted to one side that the left angle of it is nearly on a line with the septum of the nose. He has no other paralytic symptom. Several years ago he went to bed one evening with violent headach, and awoke next morning with his mouth in this state. It has continued so without the least improvement, and without any return of head symptoms. From many circumstances in the pathology of the brain, I think there is reason to believe that such partial paralytic affections are originally connected with inflammatory action in a small part of the brain; that such diseases may go on to more extensive inflammation, or may soon subside, leaving a small part of the brain indurated and deranged in its functions. This may then continue for a long time in the state of simple induration, without producing farther disease, and may at last pass into suppuration. This probably occurred in a man mentioned by Rochoux.* After a febrile attack he lost the sight of his left eye, and five years after died of apoplexy. Extensive extravasation was found as the immediate cause of his death. In the left corpus striatum there was a very small cavity, surrounded by a thick and firm sac, and containing a small quantity of a yellow fluid. There was another still smaller in the same situation on the right side. A man, mentioned by Morgagni, had headach, with loss of speech and weakness of the muscles of one side of the neck. He died after some time without any other paralytic symptom, and an abscess was found in the corpus striatum. In many cases in which the speech has been chiefly affected, the disease has been found to be in one of the corpora striata; but in a remarkable case which I have mentioned, which began with loss of speech, and went on to fatal coma, with hemiplegia, it was in the upper part of the hemisphere, in contact with the membranes. In a case mentioned by Bonetus, which was fatal by effusion, without any organic disease, one of the first symptoms was inability to raise the eye-balls. Inability to open the eyelids is frequently observed, and also, though less frequently, inability to shut them. In the same manner we meet with loss of particular senses, as sight and hearing; more rarely of smell, and sometimes of taste. A man, mentioned by Portal, was blind after coming out of an apoplectic attack, and recovered his sight after a considerable time. I know a lady who lost her sight completely several years ago, after an attack which was of an apoplectic kind, but very slight and transient.

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* Rochoux, Recherches sur l'Apoplexie, p. 40.

In the general pathology of paralysis there is great obscurity; and various and very different are the morbid conditions of the brain which are observed in paralytic cases. We have seen the disease connected, 1. With extravasated blood; 2. With serous effusion, often in very small quantity; 3. With simple and recent inflammation of a small part of the brain; 4. With encysted suppuration; 5. With induration of a part of the brain; 6. With that soft, corrupted, and broken down appearance which has been called "sphacelismus cerebri." To these may be added the remarkable case of a man mentioned by Mr O'Halloran,* who, after an injury of the head, lost a great part of the frontal bone on the right side. The bone had been completely broken to pieces. Some of the pieces were extracted immediately after the injury, and others were discharged after a day or two. A great opening was thus formed, and extensive suppuration having taken place, there were discharged through it at each dressing immense quantities of purulent matter, mixed with large pieces of the brain; making after some time "a frightful cavern" in the very substance of the brain. On the 8th day of this terrible disease the left hand and arm became paralytic, and the left thigh and the leg on the 10th; the man lived to the 17th, retaining his faculties to the last; and having been through the whole course of the disease perfectly composed and intelligent, and his pulse quite natural. No account is given of the dissection or of the actual loss of cerebral matter in this remarkable case, but some idea may be formed of the extent of the disease from the reports. On the 8th day Mr O'Halloran remarks, "The sore continued to discharge greatly, insomuch that when I affirm, that not less than three ounces of the brain, with a horrid smell, followed every dressing, I am certain that I am a good deal under the quantity." And, again, on the 13th day, "the cavern now was terrible, and I feared that the remains of the lobes of the right side of the brain would follow." The only remarkable symptom in this case was the paralysis of the left side; there was no loss of any other faculty, and "he preserved his intellect to the very moment of dissolution."

These various and opposite conditions of the brain, it must be confessed, leave us very much in the dark in regard to the pathology of paralysis, especially when we recollect farther, that we have seen them all take place without paralysis. They seem to give no countenance to the idea of paralysis depending upon pressure, but all point at certain changes in the organization of the brain, the particular effect of which upon the nervous system will probably ever elude our researches. In Mr O'Halloran's case there was the very reverse of pressure, there being a great opening in the bone, and an extensive loss of substance of the brain.

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* O'Halloran on Injuries of the Head, p. 103.

OF LETHARGY, &C.

The various forms of comatose affections which have been described by systematic writers, are merely varieties in degree, or modifications of the disease, of little practical importance. *Lethargus* is a state of torpor, with loss of memory, without constant sleep. The patient can be roused so as to answer questions, but remembers nothing, and when left to himself generally sleeps. *Cataphora* has more the appearance of constant sleep, but a sleep from which the patient can be roused, though he immediately relapses into it. *Carus* is sleep from which he cannot be roused, the breathing soft and natural. *Apoplexia* is carus, with stertorous breathing, the limbs relaxed; *Catochus* is apoplexia, with convulsive rigidity of the limbs. *Coma vigil,* or *typhomania*, is the state of insensibility without sleep, generally with delirium, which occurs in severe cases of typhus fever. These are in general nothing more than systematic refinements. Some of the conditions here alluded to, however, are deserving of some attention. The state of lethargy presents some interesting phenomena, in regard to the extent in which it may exist without passing into apoplexy, and without permanently injuring the functions of the brain, though they are for the time completely overpowered and suspended. A man mentioned by Mr John Bell, who had been accustomed to a life of much activity, was confined from his usual employments by an extensive fistula, which he had concealed. Being of a full habit, and his appetite unimpaired, he soon sunk into a state of complete lethargy. Nearly his whole time was spent in sleep. When roused he attempted to answer questions, but his answers were incoherent, and his speech inarticulate. He had been a long time in this condition when Mr Bell saw him. His fistula being cured, he recovered gradually by evacuations, blistering on the head, and a proper regulation of his diet, and in a few weeks was well. He returned to his former employments, and managed with correctness the affairs of a company.* Hippocrates mentions a priest, subject to annual fits of gout, in whom the paroxysm terminated regularly, for several years, in a state of lethargy, from which he could only be roused to take food or drink. It was accompanied by tremors, stupor, and forgetfulness, immobility of the eyes, and a completely enervated state of the whole body: it generally continued one or two weeks. A man mentioned by Willis, at the crisis of a putrid fever, lay for four days in a state of profound sleep, from which nothing could rouse him. He then came out of it, after blistering, but his faculties were gone, so that he knew nobody, remembered nothing, and understood nothing, " vix supra brutum saperet." He continued in this state for two months, and then gradually recovered.† Some years ago I saw a young man, who, at the end of a tedious fever, fell into such a degree of stupor, that I apprehended effusion in the brain. He recovered, however, after a good many days, and his bodily health was soon restored, but his mind was in a state approaching to idiotism. In this condition he was taken to the country, and recovered gradually after several months. But the most remarkable case of this kind is related in an American periodical work, which I have not now by me, and cannot refer to. The patient was a clergyman, about 30 years of age, a man of learning and acquirements, who, at the termination of a severe illness, I believe a tedious fever, was found to have lost the recollection of every thing, even the names of the most common objects. His health being restored, he began to acquire knowledge exactly as a child does. After learning the names of objects, he was taught to read, and after this began to learn the Latin language. He had made considerable progress, when one day, in reading his lesson with his brother, who was his teacher, he suddenly stopped and put his hand to his head. Being asked why he did so, he replied, " I feel a peculiar sensation in my head, and now it appears to me that I knew all this before." From that time he rapidly recovered his faculties.

The state of the brain in such cases differs from apoplexy, but it is nearly allied to it, for it sometimes occurs as the prelude to apoplexy, or it may be left as a consequence of it, after every other symptom has been removed. A gentleman mentioned by Wepfer was seized with hemiplegia of the right side, and profound sleep; in the second day the right side was convulsed, and after this the palsy disappeared. He lay in a state of sleep for nine days, having, during seven days, refused to take any food. On the eighth he began to take what was offered him, and on the ninth came out of the state of stupor, but his faculties were gone; he knew nobody, and neither remembered nor attended to any thing. After several weeks, he began to know his most particular friends, then began to remember words, to repeat the Lord's prayer, and to read a few words of Latin, (rather than German, his own language,) but only a few words at a time. If urged to read more, he said that he formerly understood these things, but now did not. He could write, however, and frequently wrote lines, both of German and Latin words, in elegant characters, but without meaning. After some time, he began to pay more attention to what was passing around him, and to look after his household affairs. He often lamented his want of understanding, and expressed his hopes that he would recover it. While thus making slight and gradual progress, he was, after three or four months, cut off by an attack of apoplexy.*

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* Bell's Principles of Surgery, Vol. II. p. 661.

† Willis, de Anima Brutorum, p. 226.

This condition of the mental faculties, we have seen, is not confined to apoplectic affections, but also occurs in other diseases, especially fevers. A state analogous to it may take place from diseases of simple exhaustion. Many years ago I had under my care a lady, who, from a severe and neglected diarrhœa, was reduced to a state of great weakness, with remarkable failure of her memory. She had lost the recollection of a certain period, about ten or twelve years. She had formerly lived in another city, and the period of which she had lost the recollection, was that during which she had lived in Edinburgh. Her ideas were consistent with each other, but they referred to things as they stood at the period of her removal to Edinburgh, or before it. Her son, for example, who was 13 or 14 years of age, she spoke of as an infant. She recovered her health after a considerable time, but has remained in a state of imbecility, resembling the dotage of old age.

Numerous examples are on record of long-continued sleep. Several cases of it are described in the Memoirs of the Royal Academy of Sciences for 1713. A man in La Charité slept four months: and a Dutchman slept six months: he then awoke, conversed for some time, and fell asleep again. No farther account is given of him.† Many of these cases, I suspect, have been embellished, and others have originated in imposture.

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* Wepfer, Historia Apoplecticorum, p. 242.

† See also Philosophical Transactions, Vol. XXV. p. 2177.

Allied to this subject is the very interesting topic of *apparent death*, which presents too wide a field to be entered upon at present. Many cases are on record of persons recovering after they had been for some time considered as dead, and even during the celebration of the funeral rites. Many of these have been cases of extreme exhaustion from fevers, especially plague, * and others have been apoplectic cases. Zacutus mentions a man who fell down apoplectic, and, after being considered as dead for 20 hours, was carried out for burial. During the procession, the bearers hearing a noise in the coffin, opened it, and found froth at the mouth of the body. Zacutus being called, found pulsation in the arteries, and the man recovered. † A woman who was supposed to have died in a state of coma, connected with hysteria, recovered after Vesalius had made several incisions for the purpose of examining her body; she afterwards died of the wounds. ‡ A similar circumstance is related of a Spanish physician, who, in opening the body of a nobleman, found his heart palpitating. A lady, sister of the great Duke of Marlborough, after being for some time in bad health, fell down suddenly, apparently dead, and was considered as dead both by her physicians and friends, except her husband, who from some circumstance that had occurred to him, would not allow her to be buried, until he should have indubitable evidence of her death. After lying in this state for seven days, she awoke as from a sleep, and enjoyed good health for many years. §

OUTLINES OF THE TREATMENT OF APOPLEXY.

The facts which I have mentioned in this paper, have an immediate and important reference to the treatment of simple apoplexy. We have seen that the disease may exist in its most violent form for a very considerable time, and yet be fatal, without producing any evident change in the organization of the brain. This occurred in case 6th, which was fatal in twenty-four hours, and in Dr Powell's case, which was not fatal till the third day. Dr Stark mentions a man, aged 31, who lay forty-five hours in perfect apoplexy, and then died, and, on the most careful examination, no morbid appearance could be detected in any part of the brain.* We have farther seen reason to believe, that those cases which terminate by effusion, and many of those in which we find extravasation of blood, were at the commencement in this state of simple apoplexy. We have seen, finally, that we have no certain mark by which we can ascertain the presence of effusion, but that some cases which have very much that appearance, are fatal without effusion, and that others may be saved by copious blood-letting. All these considerations give the strongest encouragement to treat the disease in the most active and persevering manner; not to be influenced by the hypothetical distinction of apoplexy into sanguineous and serous; not to despair, though we do not see an immediate effect from our remedies; and finally, not to be hasty in concluding that the disease has passed into a state, in which it is no longer the object of active practice.

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* See Hildani Opera, Cent II. Obs. 95. Gregorius Horstius, Lib. 7.

† Zacuti Praxis Medicin. Admirand. p. 15.

‡ Lancisius de Subitaneis Mortibus, Lib. I. Cap. xv.

§ Taylor on the Danger of Premature Interment, p. 43.

In the apoplectic attack, our first object is to take off the impulse of blood from the arteries of the head, in the hope that, in the state of collapse thus induced, the vessels may resume their natural relations, and restore the healthy state of the circulation. This is to be done by large and repeated blood-letting, purgatives, and the application of cold to the head, aided by a raised position of the body, cool air, and the absence of all stimuli. Small doses of tartrate of antimony, from its known effect in restraining vascular action, may in some cases be used with advantage as an auxiliary, provided, in the early stages, it do not excite vomiting.

It is evident, that, to produce the intended effect, the bleeding must be such, as powerfully to affect the system, producing weakness of the pulse and paleness, and it should be repeated at short intervals, as soon as these effects begin to subside. Perhaps the first bleeding should be from the arm, from a large orifice, so as to make an impression upon the whole system; but there is an evident advantage in bleeding from the temporal artery, as thereby I think we make a more immediate impression upon the carotid. Perhaps, in an urgent case, the best practice would be to bleed from the arm and the temple at once. Much importance has been attached to bleeding from the jugular vein, as most likely to give immediate relief to the head: but we must recollect, that the only jugular vein that we can open is the external jugular, which brings the blood from the integuments of the head, and has no connection with the brain, except by a very small branch coming through the orbit from the cavernous sinus, and another, equally minute, which accompanies the meningæal artery. Bleeding from the jugular vein must therefore be inferior in efficacy to bleeding from the temporal artery. Bleeding, which does not affect the system, such as that from a few leeches, can be considered as little better than a placebo. As soon as possible after the bleeding, means are to be taken for inducing strong purging For this purpose, the most active purgatives are to be given, if the patient can swallow ; if not, strong purgative injections. When purging can be speedily accomplished, the effect is often extremely beneficial. Several cases are mentioned in this paper, in which little effect seemed to be produced by the bleeding, but an evident improvement took place after a full evacuation of the bowels. The powerful application of cold to the head seems to be a remedy of considerable efficacy. It should be applied by a full stream of water directed upon the crown of the head, and received in a basin held under the chin, the patient being supported in a sitting posture. In a former paper, I have given an example of a girl restored in a few minutes, or rather seconds, by this remedy, from a state of perfect apoplexy.

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* Works of Dr William Stark, p. 73.

The use of these remedies is sometimes followed by an immediate removal of the apoplectic state. In other cases, though little immediate effect may be produced, by a persevering repetition of them, the coma begins to subside after some time, perhaps a good many hours, or even a day or two. But, in some cases, they may be used in the most active manner, so as to reduce the system as far as appears expedient or safe, without diminishing the coma ; and after all, we may find upon dissection, that the disease was still in the state of simple apoplexy. This important fact cannot be too often repeated, or too carefully kept in mind, and it should lead us to prosecute the treatment of apoplexy with the greatest attention and perseverance. "It ought to be known, (says Dr Cheyne,) that from six to eight pounds of blood have been taken from a person by no means robust, before the disease, which ended favourably, began to yield." *

In saying that our practice is not to be influenced by the hypothetical distinction of apoplexy into sanguineous and serous, I by no means intend to maintain, that every apoplectic case is to be treated precisely in the same manner. In the extent of our evacuations, a due regard is certainly to be had to the age and constitution of the patient, and to the strength of the pulse ; but

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* Cheyne on Comatose Diseases, p. 61.

I think I have grounds for saying, that there are no symptoms which characterize a distinct class of apoplectic affections, requiring any important distinction in the treatment; or in other words, a class which, in their nature, do not admit of blood-letting. On this important subject, I think I may refer with some degree of confidence to the facts which I have related in this essay. Weakness of the pulse, and cadaverous paleness of the countenance, I have shewn to be frequent symptoms of sanguineous apoplexy in its most hopeless form. On the other hand, I have given reasons for believing that serous effusion is a termination of simple apoplexy, and that cases may terminate in this manner which are accompanied by strong pulse, and flushing of the countenance. I have described a remarkable case, (case 8th,) in which there existed every circumstance that could lead us to consider the disease as serous apoplexy, but which was fatal without any effusion; and another, (Mr Turner's case,) in which there was extensive effusion, without any apoplectic symptom. Finally, I have given several examples of perfect apoplexy in persons, old, feeble, and withered, who were saved by copious and repeated blood-letting. There may, without doubt, be apoplectic cases that do not admit of blood-letting, but such cases there may also be of pneumonia or enteritis. these do not affect the general question; they are to be determined by the judgment of the practitioner, and it is impossible to lay down any general rules concerning them. The strength of the pulse is a very uncertain guide, for, in several cases that have been mentioned, in which it was at first weak, it improved after blood-letting, and continued of good strength through the farther progress of the disease. The comatose affections which I have mentioned, that are connected with an exhausted state of the system, are certainly to be kept in mind as exceptions to these general observations. These, however, are of rare occurrence; they appear chiefly in children, are easily distinguished by attention to the history of the disease, and have no influence on the general question of the treatment of apoplexy.

By bleeding, and other evacuations, we perhaps cannot properly be said to cure apoplexy. We only remove certain obstacles to the cure, which consists in the vessels resuming their healthy action after these obstacles are removed. Now we have every reason to believe, that all may be done which we can do by evacuations, and yet the vessels not recover their action. Having therefore carried these remedies as far as we judge safe or expedient, our next object is to inquire whether we have any means which may contribute to restore the healthy state of the circulation in the brain. Perhaps blistering may have some effect, and I think I have seen decided benefit from strong friction of the body. But these remedies are not much to be relied on. The use of emetics in apoplexy is a practice to be mentioned with much caution. We have been a good deal in the habit of ascribing it to Dr Fothergill, who used emetics on the principle of apoplexy arising from the stomach. But the practice does not rest upon this untenable hypothesis; it is in fact as old as the days of Aretæus, and has been employed at different times by physicians of the first eminence, among whom may be mentioned Etmuller, Sydenham, Boerhaave, and Lieutaud. It probably, therefore, must have some foundation in observation and experience. On the other hand, no person can doubt, that an emetic given in the early stage of apoplexy, would be a practice of the most dangerous kind, and most likely to convert a case of simple apoplexy into extravasation or effusion. If the remedy, therefore, is ever to be employed in any apoplectic disease, it is probably in that state which led me to these observations,—the state in which the system has been reduced as far as appears safe and expedient, by large and repeated evacuations, and yet the coma has not been removed. In this case, the operation of a mild emetic would probably be free from danger; and it is deserving of inquiry what effect it would have upon the circulation in the brain. The same observations apply to the use of stimulants of various kinds, which we find recommended by some of the older writers. The use of stimulants at an early part of the disease must be decidedly and highly injurious. But perhaps we may make a distinction between the action of stimulants in a vigorous and plethoric state of the system, and their action, when the system has been reduced by large and repeated evacuations. In sudden sinking of the vital powers, which we sometimes meet with in inflammatory disorders, particularly of the bowels, I have frequently given large quantities of wine with the happiest effect, almost immediately after a violent inflammation has been subdued, and I never saw the inflammation renewed by it, nor any bad consequence follow the practice. I imagine there are conditions of apoplexy in which stimulants might be given with safety and advantage, but the practice requires much caution.

The observations which I have now made in regard to apoplexy, apply equally to the earlier stages of paralysis. The older cases of paralysis also open a most interesting field of inquiry. Perhaps we have been too much in the habit of believing that paralysis of any considerable standing depends upon a fixed and irremediable disease of the brain. Many cases are on record which tend to shake this opinion. We see recent cases of it completely carried off in a few days,—and others recover gradually, so as in a few weeks or months to leave no trace of the disease. In many cases again, in which, after long continued palsy, the patient has died of some other disorder, we find no such fixed disease. In some, we find very little morbid appearance, and in others, only serous effusion in no great quantity. Add to these facts, the singular examples of very sudden recovery, even in cases of long standing. A man mentioned by Dr Russel, * after an apoplectic attack with hemiplegia, recovered the use of his arm in six weeks, but the lower extremity remained perfectly paralytic. After twelve months, in which he had made no improvement, he was one day astonished to find that he had some degree of motion of the leg, but it continued only a few minutes. On the same evening he had headach, and in the night was seized with a sort of fit, in which the paralytic limb was strongly convulsed. After the fit, he had slight power of moving the limb. The fit returned next day, and again in the night, and then left him completely free from paralysis, and in perfect health. He had continued well for eight years at the time when the account was written. A case considerably similar, though of shorter standing, occurred to a friend of mine. A middle-aged man was suddenly attacked with hemiplegia, and loss of speech, while he was using violent exercise, in walking very quick or running. All the usual practice was employed without any improvement, for a month. The paralytic limbs then became one day suddenly convulsed, and when this subsided, the paralysis was gone. In a woman mentioned by Dr Home, † hemiplegia of considerable standing was cured by an attack of fever.

A man, whose case is described by Mr Squire, ‡ had been liable to convulsions from his childhood till he was 25 years of age. The fits then left him, and he enjoyed good health for three years, when, without any previous complaint, except a cold, he suddenly lost his speech. He had no other paralytic symptom, and was otherwise in good health, but continued perfectly speechless for four years. He was in general a man of temperate habits, but having at this time been one evening much intoxicated, he fell from his horse three or four times in his return home, and was at last taken into a house on the road, and put to bed. He soon fell asleep, and had a frightful dream,

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* London Med. Obs. and Inq. Vol. I. p. 296.

† Clinical Experiments, p. 264.

‡ Philosophical Transactions, Vol. XLV. p. 148.

during which, struggling with all his might to call out for help, he did call out, and from that moment recovered his speech perfectly. A young woman, mentioned by Dr Watson, * had been long liable to severe convulsions, the attacks of which were frequently followed by temporary paralysis of particular muscles, which had been most severely affected, for different parts of the body were affected at different times. After one attack she lost her sight completely for five days. At length, after one of the fits, she lost her speech, and recovered it after a short time; but the convulsion returning soon after, was again followed by loss of speech, and she continued perfectly speechless for fourteen months. During this period she had no return of the convulsion, and was otherwise in good health. Having one evening violently heated herself, by dancing for four hours, she recovered her speech, and from that time continued free from complaint.

These examples point at a most important principle in regard to the treatment of paralysis; that cases of it, even of long standing, sometimes depend upon a cause which is capable of being removed completely, and removed almost in an instant; and they hold out to us a most interesting subject of research in the treatment of such cases, which, of all diseases, are usually considered as the most hopeless.

After the first urgency of the symptoms has been subdued by the means that are employed in the apoplectic attack, the restoration of the paralytic parts has been attempted by a variety of remedies, chiefly of a stimulating nature, both external and internal. To the former class belong warm-bath, friction, electricity and galvanism, to the latter mustard, ammonia, camphor, and nearly the whole class of stimulants. It is not easy to decide upon the merits of these remedies; but it is certain, that they all require to be used with much caution, as the action by which they have the chance of being useful, is nearly allied to that by which they may renew the apoplectic attack. Perhaps, on a principle to which I have already alluded, the danger from the use of them may be in some measure averted, by keeping the system very low, by spare living and evacuations. This, I imagine, is always to be considered as an essential part of the cure. Paralysis is something very different from debility; and I cannot agree with some most respectable writers, who hold, that the diet in paralytic cases should be nourishing and restorative. With this precaution constantly in view, I think it probable that

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* Philosophical Transactions, Vol. L. p. 743.

there are cases of paralysis in which stimulants may be employed with benefit. I cannot say what remedies of this class are to be preferred. Dr Vaughan has strongly recommended the tincture of cantharides, * others have employed the balsams and turpentine, mustard, Arnica montana, guaiacum, seneka, and various others; also several articles of a narcotic quality, as the Rhus toxicodendron. In Germany, phosphorus is said to have been lately given internally with advantage, and in France, the favourite remedy at present is the Nux vomica. † It is given in extract, in doses of gr. ij. three or four times a-day. It is apt to occasion convulsion, and the first proposal of it is said to have been founded on the observation, that when paralytic limbs become convulsed, they frequently soon after recover their power. In the most favourable examples, however, that have been given of its efficacy, a long time was required for the recovery; and as we know that a considerable proportion of paralytic limbs recover spontaneously, we must be very cautious in ascribing recovery to the action of any particular remedy. Emetics have also been strongly recommended, and mercury pushed to smart salivation. Mr Wardrop ‡ has described a singular case of eighteen months standing, which seemed to derive benefit from tickling the parts with a feather: it recovered in two months: and M. Gros mentions a cure by stinging with nettles. § Celsus seems to have employed a similar practice.

The cases of inflammatory paralysis are to be treated upon the same general plan as the more common form of the disease. They ought to be treated with great decision at the commencement, as irremediable mischief may probably be done to the brain at a very early period. The discharge by blistering, issues, and topical bleeding, may perhaps be more beneficial in these than in the apoplectic cases.

Symptoms indicating a tendency to the apoplectic or paralytic state, are to be treated upon the same principles, by evacuations, spare diet, abstinence from stimuli, and from all exertions that quicken the circulation, cold applications to the head, shower-bath, &c. Frequently we make little impression upon them by regimen, until we have given them a turn by one full bleeding. Dr Cheyne has recommended preparations of anti-

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[*] Mem. of the Med. Society of London, Vol. I. p. 360. See also Edin. Med. Journal, Vol. X. p. 419.

[†] Bulletins de la Faculté de Medicine, 1816, 1817.

[‡] Edin. Med. Journal, Vol. VIII. p. 197.

[§] Mem. de l'Acad. Roy. de Sciences, 1741.

mony as very beneficial in the apoplectic tendency. He employs James's powder, of which he gives a dose every night—he has also found it useful in epilepsy. * Perhaps we pay too little attention to the influence of sleep upon the functions of the brain. I imagine that great abridgment of the quantity of sleep might be found, in affections of the head, a more powerful remedy than we are aware of.

II.

Copy of a Report of the State of Health and Disease of the Royal York Rangers, at St Joseph's, Trinidad, for the Quarter ending the 24th September 1816 *; transmitted to the Army Medical Board through the Inspector of Hospitals in the West Indies.* By James M'Cabe, Surgeon of the Royal York Rangers.

The Royal York Rangers at this station having, during the months of August and September, been more than usually unhealthy, and a report at the same time prevailing that a fever apparently contagious had made its appearance in some of the West India Islands, the greatest attention was paid to the state of disease in this regiment, in order to ascertain, if possible, if there were any thing epidemic in its character.

The weather during this period was, as is usual at this post, almost continually wet, but the intervals of showers were marked by a more intense degree of heat than had occurred at any other period since the regiment had arrived in the island. The almost continual rains had prevented the brushwood which surrounds the barracks and hospital from having been cleared away. The intense heat of the sun, during the intervals of showers, occasioned an increased degree of exhalation from those sources of effluvia, and from the swamps and marshes which are contiguous to this post, and rendered the atmosphere more than usually impure; whilst scarce a breath of air relieved the oppression occasioned by the sun's heat, or communicated its motion to the sultry and stagnant atmosphere.

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* Dublin Hospital Reports, Vol. I. p. 315.

About one hundred patients (being about one-fifth of the whole of the regiment in the island) were, during this period, the average number in hospital. The remittent fevers were in general less inflammatory than those which usually occur in this regiment. They were in general characterized by great debility,—by a paleness of the face,—a shrunk and dejected countenance,—a feeble and unequal pulse,—a general excitement, and great irritability of the nervous system,—a coldness of the skin; the heat and blood appearing to retire from the surface to the internal and vital organs. A low delirium or stupor appeared early, and attended them throughout their progress. Such as put on these appearances were the most dangerous. Some were attended with greater action of the sanguiferous system and determination to the head, with violent delirium. Such as assumed this form in general terminated favourably.

The absence of inflammatory symptoms in most of the fevers at present, appear to be in a great measure occasioned by a change which has taken place in the constitutions of many of the men since they came to this island. Many who were then of plethoric habits and robust constitutions, are now of a pallid sickly hue, and dropsical diathesis. This change of habit and constitution has, I understand, invariably taken place in every white corps that has hitherto occupied this post for any considerable time.

The unfavourable and impure state of the atmosphere, during the month of August and September, not only occasioned an aggravation in the character of the fevers, but also produced an unfavourable effect on all the diseases in the hospital. The chronic cases of disease, dysentery, anasarca, phthisis pulmonalis, &c. which, although there could be no hopes of curing, might in a more healthy season linger out some time longer, owing to the above causes dropt off in a more than usual proportion. The ulcers became foul and sloughing, and they and the cases of fever appearing mutually to affect each other, it was found necessary to occupy a part of the barracks for the accommodation of surgical cases.

From these considerations I am inclined to conclude, that, whatever increase of sickness, or even of mortality, has occurred at this station within the last two months, it may be accounted for by the unfavourable state of the weather and atmosphere, occasioning aggravated forms of disease in constitutions that have been injured by local peculiarities, and which are always under the influence of irregularities and intemperance; and, consequently, that the fever which occurred at this station is an aggravated form of the endemic fever of the island, having nothing epidemic or contagious in its character.

John Humphreys, of the Royal York Rangers, 32 years of age, of a robust constitution and plethoric habit, was admitted to hospital on the 17th August 1806. He complained of pains in his back and extremities; pulse 120, full and strong; heat 99; skin dry; face flushed; eyes inflamed; great throbbing of the temporal arteries, and determination to the head; tongue foul; bowels constipated; great excitement, and irritability of the system. Three pounds of blood were taken from the temporal artery, his bowels opened with a scruple of calomel and 1½ ounce of sulphate of magnesia. He had afterwards a warm bath, and 10 grains calomel and six James's powder at bed-time. On the 18th, 2d day of his disease, the symptoms were somewhat moderated; skin hot and dry; headach still severe. Calomel. gr. v. pulv. Jacob. gr. iij. et ℥j. aq. acetat. ammoniæ quater indies; warm bath. On the 3d day delirious; great irritability of the nervous system; pulse 110; heat 96; tongue parched; eyes sunk; countenance expressive of great anxiety; stomach sick. ℞ Camphoræ et submuriatis hydrargyri āā gr. iv. opii gr. ss. M. Fiat bolus 4ter. indies capiendus. Haustus effervescens post singulos bolos. Applicentur emplastra lyttæ capiti, et scrobiculo cordis. The bowels were kept open by frequent enemas, plenty of diluents, such as tea, toasted water, rice water, &c. were given him, and varied to his taste. On the 4th his bowels not being sufficiently open ℥i. olei ricini was given, which remained on his stomach and produced the desired effect. The boluses were continued; the great excitement of the system appeared gradually to abate; his intellects became clearer; the pulse soft and regular, with a gentle moisture on the skin, and on the 6th day of the disease he was free from fever. His progress to convalescence was regular, and on the 11th September he was discharged from hospital. This case of fever was attended with more inflammatory action than any other that occurred during the period.

Sergeant Haigh of the Royal York Rangers, aged 34, of a spare habit, and weakly constitution, was admitted to hospital on the 25th August 1806. He had been exposed to a great deal of fatigue on the day before, during very bad weather, and had remained in wet clothes the greater part of the day. When admitted, he complained of headach; pains in the back and loins; pulse frequent and feeble; skin hot and dry; countenance anxious amd dejected; tongue parched; stomach sick. He was bled from the arm, but syncope occurring, only 20 ounces were taken away. He had calomel and salts, which operated well, afterwards a warm bath, and calomel and James's powder at night. 27th, 2d day of the disease. Pulse frequent, small and feeble; skin cool; eyes languid; countenance pale and anxious; stomach sick, and rejects even the arrow root and tea;

thinks himself better; bowels open; tongue dry. ℞ Calomel gr. iv. opii gr. ss. Fiat pilula 3tiis horis capienda. Haust. efferves. post sing. pill. Applicetur scrobiculo cordis emp. lyttæ. Enema opiatum bis in dies. These remedies were continued during the day, but the irritability of the stomach continuing and increasing, he could not retain either the medicine or nourishment. Incessant vomiting at length occurred, notwithstanding the use of opiates, enemas, and fomentations. He became gradually exhausted, and died about three o'clock in the afternoon, about 48 hours afer he had been received into hospital.

Dissection of Haigh.

Adhesion of the lungs to the pleura.—The liver covered on its convex surface with a white and firm membrane of coagulated lymph, forming a strong bond of connexion between itself and the abdominal parietes. The inner surface of the stomach was bedewed with a whitish-coloured viscid fluid, not unlike pus, probably coagulated lymph, effused from the inflamed vessels. Livid spots on several parts of the stomach and colon. The great arch of the colon was a gangrenous mass.

Lieutenant ——— of the Royal York Rangers, about 26 years of age, of a spare habit and weakly constitution, found himself unwell on the 4th September 1806. He complained of headach; pains in the back and loins; great prostration of strength; pain in the chest, attended with cough and difficult respiration; pulse accelerated; nausea and thirst. He had been shooting in the woods the day before, and had been exposed to torents of rain. He was bled to the extent of 32 ounces; a blister was applied to the breast, and his bowels were well evacuated with calomel and salts. On the 2d day of the disease, pulse 100, and feeble; heat 90; tongue foul; bowels open; headach relieved. ℞ Calomel gr. v. Pulv. Jacobi. gr. iij. M. Capiat quater indies. Balneum tepidum. These remedies were continued during the day, and on the 3d of the disease he was free from fever. He continued free from fever during the day; but in the evening he went to a gentleman's house in the village of Saint Joseph's, where he imprudently drank some porter, and was in consequence again taken ill, and brought to his quarters in a state of delirium. Pulse feeble, frequent, and unequal; face pale; countenance anxious; great excitement and irritability of the system. ℞ Submuriatis hydrargyri et extracti cathartici singulorum scrupulum. M. et capiat statim. Capiti raso, et cum aceto lavato, applicetur emplastrum lyttæ. Enema purgans cum ol. ric. ℥ii. statim adhibeatur. He continued delirious during the night, and was kept in bed only by force. On the 4th of the disease still delirious; restless; picking at the bed-clothes; bowels open; pulse about 100, tremulous, and unequal; heat 86; countenance anxious and dejected. ℞ Submuriatis hydrargyri et camphoræ āā. gr. iv. opii gr. ss. M fiat pilula quater in dies accipienda. Haustus effervescens post singulos bolos. Enema ex amylo cum tinctura opii frequenter adhibeatur. He remained delirious during the day, without any amendment. The only favourable symptom was that his stomach retained the medicines and some tea and sago. He continued delirious during the two following days. As his delirium was of the low comatose kind, wine was given him. On the 5th day of the disease cataplasms of the capsicum annuum, pounded into a mass with flour, were applied to his feet. The boluses of camphor, &c. were continued. His delirium abated on the evening of the 5th; his intellects became clearer; his pulse softer; skin moderately warm and moist; he slept several hours during the night, and on the morning of the 6th day of his disease or 4th from the 2d attack he was free from fever. Although extremely debilitated and exhausted, he continued to improve, and is now (24th of September) in a state of convalescence.

From the 1st of August to the 20th of September 45 cases of remittent fever were admitted to hospital, and the case of the officer already detailed, make 46 within this period. Of this number five died. The others are now convalescent, or under treatment, and likely to do well. Those who died were mostly men that had suffered from former diseases, and whose viscera, there was reason to think, had been much diseased. The other cases that occurred were similar to those that have been detailed, and the mode of treatment the same, allowing some variation for variety or peculiarity of symptoms. The James's powder was, in some cases, observed to increase the general sickness and uneasiness. and to occasion sickness of the stomach. Camphor, calomel, and opium, with effervescing draughts, were the remedies that appeared to be attended with the greatest benefit, and seemed to remove, better than any other medicine, the general restlessness and excitement of the system, which, whether the causes or consequences of the disease, always attended it. It was also remarked, that the action of mercury on the system did not supersede the action of fever, as some of the men who died were under the influence of that medicine.

III.

Observations on Small-Pox, Natural and Modified, as they have appeared lately in Newton-Stewart. By James Black, Surgeon.

At no time since the introduction of vaccination, has the faith in its efficiency been so much threatened, both in the minds of many medical men, and in the public opinion, as it has been of late in various parts of this kingdom. Since the epidemic revolution of the small-pox has pervaded the country, the most confiding of the advocates of the Jennerian system must have remarked some phenomena they did not anticipate, and which would tend to puzzle a little the enthusiasm of their minds. Nosology has been again revised, and found unsatisfactory, and at some variance with observation; while a definite description of these genera of exanthemata has in vain been looked for, as a sure guide to diagnosis. Amid a good deal of doubt, some scepticism, and much gratuitous confidence, the minds of the uninformed community have not regardlessly looked up to the vacillating countenances of their medical superintendents, and have been more ready to seize upon any thing tending in the least to the prejudice of vaccination, and to court distrust in a remedy that had no origin in popularity, than they have been to take the physician's reserve for reflection, or to aid the solution of seeming perplexities by unprejudiced and ingenuous accounts of diseases of this class that may have appeared in their families.

The insulated practice of a country practitioner, excluding him for a while from the immediate knowledge of all that is happening around him relating to the nascent epidemics of a season, he had, at first appearance of this anomaly of the small-pox, much to recollect, daily doubts to resolve, and the staggering faith of others to uphold; while his own was perhaps any thing but confirmed by all he witnessed and heard rumoured; and if it was of recent adoption, and unpropped by personal experience, it perhaps would unsettle in the current that clamoured around him; until the reported observations of his brethren began to accumulate, and made him a sharer in all they felt, and in all they observed.

As the anomalous epidemic continued to spread, observations began to multiply, and the practitioner who, for a short period, saw himself involved in much individual responsibility, concerning the opinion of what is of so much importance to millions, and of so much intense interest to the medical world, now felt the question shared by the increasing experience of his brethren, the partial phenomena in his sphere elucidated by a series of others of more or less extent, and the timid conclusions of his own mind confirmed by amalgamating with those of others; while the perhaps hasty dogmas of momentary perplexity would be dissipated by the legitimate conclusions recorded around him.

In thus premising the few observations I intend to make principally on the irregular species of variola that have lately appeared in this place, I, by no means, pretend to add much of novelty to the phenomena already so well described in several publications, as well as in many papers of this eminent and highly useful Journal, or to set up any dogmatical opinion bearing on the subject; nor do I indeed aim at any new hypothesis, to surprise the orthodoxy of the practitioner, and delight the generalising ideas of the student. If, however, they tend to add the least contribution to the general promulgation of ideas on what appears to excite so much new and deserved interest, or to afford any thing corroborative to the structure of a judicious theory, the views of the writer will be amply gratified; the more so, as he has the confidence to hope, that little can be derived from them, anywise militating against the fundamental merits of the invaluable boon Jenner has conferred upon us; while not a little of what they contain may be adduced as confirmative of its superior protection, though it may be shorn a little of that splendid infallibility its admirers so fondly contemplated.

Though the appearances of this modified disease have been very well described by several observers, it may not be inappropriate to enumerate briefly some of the leading characteristics which it exhibited in this part of the country, and as they were noted in the summer of 1817.

The general features of the epidemic were exceedingly discrepant and varied in severity. They sometimes exhibited all the primary symptoms of confluent small-pox; at other times, and in other individuals, those of the mild character of what is commonly termed varicella; and between these extremes, there seemed to be every gradation, both as to pyrexia, and to the evolution on the surface of the body. Fever, in general, was, however, pretty violent, and ushered in by languor, lassitude, intense headach, much anxiety, and general pains. In some few cases, delirium and coma were observable, and, on the day preceding the eruption, an extraordinary degree of indescribable oppression about the præcordia. Vomiting obtained in many instances, and the heat of skin was constant, dry, and burning. In other cases, the fever has been so slight, as to indicate only a gentle indisposition before the eruption appeared. The fever, after continuing scarcely three full days, was not much abated on the appearance of an eruption on the evening of the third or morning of the fourth day. The first cuticular phenomena were, in many cases, a papulous eruption, with efflorescences of a fiery red, upon the face and hands. In one case, the legs and feet appeared as if beset with erysipelas, while the body was regularly spotted with unelevated papulæ, so like rubeola, that

I would have pronounced the disease to be such at first inspection, if not previously informed by the mother, that the child had had the measles. In the midst of the rash, a solitary inflamed vesicle might be observed, with a distinct margin, often on the brow, or adjoining to the hair. The anxiety and features of high fever subsided a little on the appearance of this eruption; but if the bowels happened to be neglected, the affections of the head, as coma, may remain. This precursor (once mistaken by me for the real disease) is soon followed by an eruption of small inflamed fiery red spots, though not so red as the previous deciduous maculæ. These were observed to emerge to the surface, sometimes close upon, and in the midst of the rash; at other times the rash wholly disappeared, and a fresh accession of fever was a little relieved by the true eruption upon an unoccupied skin. This eruption is generally very copious on the face, hands, and feet, where it makes its first appearance; while the body may be moderately and regularly studded at this period. This true eruption, in other cases, has been the original tenant of the skin, and less of the preceding one has appeared since the epidemic was on the decline; and the state of previous vaccination seemed to be noways connected with any influence upon it. The more obvious accompaniments of pyrexia now nearly cease, though it preserves a steady but moderated violence, as the eruption does not appear all on one day, but successive crops, for at least three days, are elicited to the surface. On the second day, the papulæ of the first crop are more enlarged, inflamed, and feel elevated to the hand, while the interstitial skin regains a more natural colour, and in the earliest, the apices may be observed furnished with a little lymph, but which is more apparent on the third day of eruption. On this day, the fever is much abated, but the face is greatly swelled and painful, eyes, in some cases, are shut up, and the throat begins to feel sore, with a little cough. The eruption now, in a majority of cases, has the decided character of pustules, which are irregular in form, but generally round, or a little oblong, having mostly a depressed centre at this stage, with edges rising boldly from the skin, and in many other parts, as the feet and legs, bladder-like vesicles of a pale dirty white may be observed, of irregular form and extent, and which often burst, leaving superficial abrasions that require dressing. On the fourth day, many of the pustules resemble those of the confluent small pox; others are not so decided in their appearance; some being in one stage, verging from the lymphatic to the pustular colour; others again are replete with lymph, while a third series are inflamed papulæ, with a continuance of slight fever. The fifth day produces new changes in the progressive states of each series, and the sixth day has fully matured many of the first crop. These may often now be remarked to subside a little, and to lose their depressed centres. In others, though the pustules seem to be fully charged with fluid pus, yet few burst and coalesce in the scabbing process, but the secreted fluid undergoes absorption, or evaporation through the pores of the pustule. On the seventh and eighth day, the subsidence is more apparent over all the crops that have successfully matured. While some of the latter ones are proceeding rapidly through all their complexions, others of the first crop are flattened, becoming dry and brown, which is the colour they preserve, with a deepening shade, until their final departure, which is about the 11th, 12th, and on to the 16th day. The throat gets easier, cough subsides, and any thing similar to secondary fever has not been remarked. Even the two cases who died, and who had undergone the process of vaccination in the same manner as others of the same family were declared to have done, were carried off at the height of the eruption.

Many cases would exhibit no pustules, correctly speaking, but hard vesicles, of more or less tubercular appearance, and these again would be more or less numerous, gregarious, or dispersed in different individuals. These were termed by the people *nerles*, or *horn-pox*, and have long been noticed by very aged matrons, who pretend to no little skill in the diagnostics of small-pox, and who have distinct varieties by name, beyond the enumeration of any nosologist. Marks are discernible in many of those who were affected with this secondary disease. They are generally superficial, and may on the young be mostly obliterated by time.

The first case seen or heard of, at this period, in the place, was by me, in the autumn of 1816, and manifested itself in a girl who came direct from London, and had an eruption of what is termed *horn-pox*, after vaccination, which, on her arrival, was of about five days standing; but the next affected, to my knowledge, was another girl, her companion in the same family; she had true small pox, her vaccination having been very equivocal. Sporadic cases continued to occur, until the ensuing summer elicited the epidemic pretty extensively.

The above were the general symptoms that seemed to characterize the epidemic, when preceded by vaccination, though many had the disease in a more or less mild form; as may be partly seen from a subjoined summary hereunto annexed, for the purpose of exhibiting the various degrees of the disease proportionately in a given number, indiscriminately taken from one end of the town, since the epidemic first made its invasion; more than to affect to shew the respective numbers that may be said to have been affected with the *peculiar specifics*, variola, varicella, *horn-pox*, *nerle-pox*, bleb-pox, modified-pox, &c. I have inserted the cases of natural small-pox, that either ended fatally, or recovered in the same district, along with those certified to be most analogous to variola, from the *standing out*, as it is termed, of the eruption. Though the whole of the 100 cases were not personally visited, and inspected by myself, at the period of disease, yet the greater number of the severer cases I attended, and with the history of the less important or milder, I have made myself well acquainted.

Summary of 100 Cases.		Vaccinated.	Inoculated by Small-pox.	Recovered.	Died.
Of small-pox,	43	10	1	30	13
Modified-horn-pox, &c.	47	44	0	47	0
And Varicella,	10	8	0	10	0
Total,	100	62	1	87	13

From this it may be seen how the epidemic has borne upon the vaccinated, and those who were not: though it must be observed, of the ten placed vaccinated opposite to small-pox, that it implies nothing certain as to the perfect and most efficient proofs of that process, only serving to indicate those who had been subjected to that process, having marks on their arms more or less equivocal, and who were said to have had fever at the time, none of these having been vaccinated by me. The severest cases of the secondary pox were observable in those from childhood and upwards; while infants seemed to suffer less: though I cannot say the severity of the symptoms increased in any accurate ratio to the distance of time that had elapsed from vaccination.

A family of several individuals exhibited, in two or three instances, all the species of what are termed variola, varicella, &c. While one unvaccinated was lying under all the loathsome phenomena of small-pox, others, who had undergone vaccination, manifested as high eruptive fever, succeeded by pox confluent on the face, and dispersed on the body, but of short existence; and a third, also vaccinated, would, after a slighter fever, display an eruption of papulæ that never suppurated, and of many that were never furnished with the least lymph; while the mother, who had, early in life, been inoculated before the Jennerian discovery, yielded an acknowledgment to the pervading power of the disease around her, a scarce sensible indisposition, to which, from two to half a dozen of pox would almost unconsciously supervene on that side of her face next to the arm upon which she carried her child in the complaint, or among her hair.

Two of the cases, in which vaccination had been performed, died. The one had a cicatrix, but it was large, and failed in complete specific physiognomy as to regularity and indentations; the other had one more genuine, yet not satisfactory to the medical attendant when examined during its illness. Both had constitutional fever at vaccination, and were cut by an experienced person, though not a regular practitioner. Constitutional fever is however often confounded with symptomatic arising from preternatural inflammation and suppuration of the vesicle, which is known to destroy the silent process essential to maturation and consequent efficiency. I may remark a singularity rather in two of the above cases, namely, the appearance of modified pox leaving marks during the summer of 1817, after vaccination two years previous, and last summer, a secondary eruption, in the same individual, of milder appearance, or what is termed varicella; and in the other case unvaccinated, small-pox was preceded, the year before, by an eruption of varicella. These cases excite reflection; but it would be attributing too much importance to them, to aim at establishing much collateral proof of specific classification. For, if we allow the variolous virus the privilege of exhibiting itself, under all the features of papulæ, vesicles, and pustules, and a varying intensity of the epidemic at the time, with the proofs we have of the constitution being twice affected with small-pox, the above anomalies may be explicable. But if we do not allow of this, but of distinct genera, succeeding each other, how shall we class the first eruption after vaccination, as it has no parallel as yet in nosology, or be able to distinguish varicella in the midst of external phenomena, but varying in degree?

As to the identity, then, of modified small-pox and varicella broached very ingeniously by Dr Thomson in a late number of this Journal, some of the preceding observations may be adduced as favouring such an hypothesis; such as the indiscriminate appearance of the whole genera in one family, and nearly at one time, produced, in this view of the subject, by a varied intensity of the contagion, constitutional liability, previous vaccination, and adventitious circumstances of exposure, confinement, diet, and previous disease. It may be asked again, How will this theory be affected by the milder disease varicella, preceding distinctly either small-pox or vaccination, as in two of the cases included in the above summary, and by those cases succeeding genuine vaccination, which, being nearly equal as to eruptive fever, exhibited, in some individuals, a very few pustules of good size, and regular procedure to early maturation, and in others, an irregular crop of small, hard, and imperfect vesicles of shorter duration? These, perhaps, by pathological ingenuity, may be accounted for in a manner not highly incompatible with the hypothesis above alluded to; but the less ingenious will find some difficulty in subscribing to it. Whatever difficulty may arise to reconcile such an identity, there cannot be much in proving small-pox and modified small-pox to be essentially the same in the nature of their virus; and their whole phenomena may be solved by a consideration of all that aggravates or ameliorates the nature of any epidemic, conjoined with the powerful controlling influence of previous vaccination. So well convinced I am of this agency of the cow-pox, that though *it* has failed in bestowing complete immunity from the small-pox contagion, it has, in the limited sphere of my observations, saved the lives of many, and preserved dozens from a critical and loathsome confinement.

I have never observed varicella without small-pox, or modified pox being in the neighbourhood, if not in the same house; and the more certain and genuine was the vaccination, the more mild, in general, was the supervening disease, especially as to the eruption—the fever in some cases having a violence uncountenanced by a corresponding severe eruption.

As to any connection between this epidemic and the typhus that has since prevailed to a small extent in this place, I can remark no other association but the one succeeding the other in point of time; but it was observed, the last time fever prevailed to any extent in this quarter, it had the same harbinger, the epidemic small-pox.

Without lengthening these observations, which, to the public, may appear trite and too diffuse, I shall presume to hope they may afford at least some additions to the accumulated remarks on this interesting epidemic, that has of late threatened to involve the *merits* of vaccination in much discussion, with many of the medical world, as well as to compromise *them* in the opinion of the prejudiced and uninformed. The vaccinated are now regaining that courage and confidence which received a temporary shock at the first invasion of the disease, which threatened, in the minds of the alarmed, a total overthrow to the protecting guardian of their constitutions. It was however found to be repulsed in its full and very dangerous character, in the vaccinated, though it continued to blaze out in all its loathsome features on those who had neglected that process. The struggle between the efforts of the fresh epidemic and the former possessor of the system, has, in some few cases, made the patient suffer nearly as much as if the small-pox had originally pervaded the constitution ; yet, if we may judge from the symptoms of invasion acting so powerfully, when met by the residing influence of the cow-pox, the entire and uncontrolled conquest of the small-pox, could not have been but very severe in its whole progress to the majority, and fatal to many.

The rationale of the appearance of this irregular disease at all upon the vaccinated, must depend, either upon the efficiency of the cow-pox not being absolute to preserve the human race from some liability to that loathsome disease,—the spurious individuality of the virus employed, or upon a new and redoubled potency in the variolous contagion. If the cow-pox, from all I have observed, had either absolutely protected the constitution from small-pox, or did not do so, it is to be presumed the phenomena would not have exhibited their present equivocal character,—either complete immunity, or small-pox, confluent or distinct, would have obtained ; but as a series of appearances have manifested themselves through every gradation, from small-pox to the mild eruption of what I consider as chicken-pox, we must look for the cause of this parasitical disease in some of those cited above. I hope, however, it shall be satisfactorily ascertained, that the spurious matter, (if such there be,) or an imperfect mode of vaccination, is the influencing medium of the whole phenomena of the severer cases, as there is every probability, from what has come under my observation, that the protecting influence of cow-pox is in some obvious proportion to the more or less perfect manner the vaccination has been conducted, or favoured in its evolution, by the absence of all those circumstances, so well known to have the power to counteract and disturb that silent, and beautifully elaborate process of nature. It may again be remarked, though no certain stress can be laid upon it as an infallible test for what may happen, that the more complete and genuine the appearance of the cicatrix itself, the more I observed the vaccinated constitution to resist subjection to the epidemic, and the more it failed in being satisfactory, the more in general were the external phenomena approximated to small-pox—so that insurance from its ravages seemed to be possessed in proportion as the vaccination had been more or less genuine in its nature and progress. If this shall be found from other and future observations, not to account for the solitary and supposed failures of the cow-pox, then we are obliged reluctantly to allow, that vaccination in its most perfect manner, bestows but an immunity from that loathsome epidemic, varying from a shade of protection to that of an insurance, neither infallible nor uniform in its proofs. But until we are driven to the diminished alternative, it is incumbent on the profession, and the public, to be most scrutinous in all possible verification in every case, of a boon that seemed to burst on the world like a specific dispensation from Heaven, and not suffer the merits of this benignant blessing to be obscured, or frittered away, by either negligence or misrepresentation, if the legislature of a country, that gave birth to Jenner, has not yet seen it expedient to guard against every thing that may in the least affect its existence, or tend remotely to compromise the munificent honour that the gratitude of a country bestowed upon its eminent discoverer.

Newton-Stewart, October 17th 1818.

IV.

On a new Method of introducing Mercury into the System for the Cure of some Tropical Diseases. By William M'Clure, Surgeon, Old Kirkpatrick.

A Favourable or unfavourable prognosis in the hepatitis and other endemics of the East, does not seem to depend so much on the comparative mildness or severity of their symptoms, as upon the greater or less susceptibility of the patient's system to become impregnated with mercury. If credit may be attached to the repeated declarations of some authors, there is hardly a symptom, however formidable, in any one tropical distemper, that does not immediately suffer an abatement, and eventually an annihilation, through the influence of that mineral; and, therefore, the speedy saturation of the system, with so very powerful a medicine, becomes an object of the very last importance in the medical practice of the East and West Indies.

It is not my intention, in this paper, to enter at all into the history of those dreadful distempers, arising from the high temperatures of tropical climates, and the marshy exhalations which are thereby produced; but, taking it for granted, that the severity of their symptoms, the rapidity of their progress, and the too frequent mortality of their result, are familiarly known, I propose merely to suggest a new mode of exhibiting that medicine, in the treatment of them, which is now almost universally allowed to be the most efficacious of any that have hitherto been employed. The great and distinguishing superiority of the method of charging the system with mercury, in the treatment of tropical disease, which I am about to describe, is founded on the extraordinary rapidity with which it accomplishes its effect. When it is considered that some of the endemics of warm climates, such as the yellow fever of the West Indies, the bilious fevers of Batavia and Bengal, and some vehement cases of dropsical hepatitis and dysentery, are of a nature so extremely violent, and have a progress so alarmingly rapid, that not unfrequently they destroy their victims, unless mercury be timeously interposed, in the short space of 18, 24, 30, or 40 hours; and that the common methods of exhibiting that remedy never produce a ptyalism, (without which no good is effected,) before the 4th, often not till the 7th or 8th day, and very frequently not at all; we will require no other argument to evince the high utility of a mode of exhibiting it, which, as the following case will show, is capable of exciting the mercurial action in the body in a space of time not exceeding the fourth part of the shortest of these periods.

The method which I allude to is, that of scarifying a portion of the skin, and sprinkling the powder of corrosive sublimate, or muriate of mercury, upon the wounded surface. This substance is soluble in the fluids of the body. Absorption, therefore, immediately takes place, and a rapid and certain effect is produced on the constitution.

The method in question, though it has frequently been employed to produce a local effect in scrofulous and other chronic affections of the joints, has never, so far as I know, been tried with the direct view of producing a general effect on the constitution.

The extraordinary rapidity with which the body may in this way be charged with mercury was well exemplified, some time ago, in the following case, the account of which I have extracted from the records of the Royal Infirmary of Glasgow.

On the 29th day of May 1816, James Wilson came into the house. His left knee-joint was much swelled, stiff, and painful. These were the sequelæ of an injury, which he had sustained a considerable time before, and for which leeches, blisters, and an issue, had in vain been employed. The issue was open when he arrived at the hospital; it was then ordered to be healed up. This was accomplished by the 10th of June; and the following extract from the books of the house will show what were the subsequent treatment and its results:—

"June 10th.—Let there be inserted, immediately, to the fibular side of the patella, an issue formed by the muriate of mercury.

"13th.—He experienced very considerable pain from the insertion of the issue. The inflammation has extended from its margin over nearly the whole fore part of the joint. Poultices have been employed. *The day following the use of the muriate of mercury, salivation to a slight degree took place.*

"21st.—The escar has separated. Since the active inflammation which it produced has abated, the swelling of the knee has decreased most wonderfully, it measuring now only an inch more in circumference than the healthy knee."

With the comparative merits of the muriate of mercury and of the nitrate of silver, as escharotics, in diseases of the joints and other parts of the body, I have at present nothing to do; all I wish to deduce from the action of the mercury, in this instance, is, that it induced its full and peculiar effect on the constitution, in a space of time incomparably shorter than that which is occupied by any other form of the medicine employed in any other way. "The day following the use of the muriate of mercury, salivation to a slight degree took place." This requires little comment. The fact speaks for itself. I may, however, subjoin one or two observations. Although in the books of the house it is not said at what hour of the following day ptyalism came on, we may presume, from the known regulations of the hospital, that the issue was not inserted, probably before three, certainly not before two o'clock, P. M.; and I remember well, that the patient was complaining very much of the pain of his gums at one o'clock next day, so that in a period of time, not exceeding 18 or 20 hours, the full and appropriate effect of the mercury was produced in this man's constitution. I need hardly add that, though this was a very disagreeable, and even a supererogatory effect, in the case of Wilson, it would have been welcomed with the highest satisfaction by the physician between the tropics, when anxiously labouring to oppose the approaches of suppuration in hepatitis, of gangrene in dysentery, or of the last and mortal symptoms of a Batavian endemic.

But the rapidity with which the mercurial action may, by this method, be produced in the body, is probably not its only superiority over at least one of those which are in ordinary use. Mercury, however cautiously administered, has always a disagreeable effect on the organs of digestion. Now surely it is devoutly to be wished that we avoid, as much as possible, irritating those delicate organs, by the internal use of medicine, which, in almost every instance of tropical disorder, are already suffering too much disturbance in consequence of disease. While, therefore, we are employing the lancet to obviate congestions of the liver and brain; the milder kinds of purgative medicines to purify the alimentary canal; antimonials to determine to the skin; and opiates to remove every kind of morbid irritation, mercury, by this plan of introducing it, may be finding its way into the constitution, and operating its salutary effect there, without injuring, in the smallest degree, either the structure or functions of the stomach and intestines. I cannot anticipate a single objection to the adoption of this practice. In the case above detailed, the excitement produced by it differed in no respect whatever, except in the rapidity of its approach, from that brought on by mercury, exhibited in any other way. Its local effect, indeed, must be allowed to be an unpleasant one in constitutional diseases; but before this can operate the rejection of the method, we must be prepared to admit, that the presence of an eschar, easily cured, upon the surface of the body, is an object more to be dreaded than an abscess immured in the substance of the liver, or gangrene affecting the alimentary canal. To obviate, as far as possible, the disagreeable effect of the eschar, such parts of the body only may be chosen for its seat, as are but seldom exposed, and on which the patient is not obliged to repose.

Before concluding, it may perhaps be proper to remark, that I by no means intend, that the foregoing mode of saturating the system with mercury, should, in *every case* of India hepatitis, or other tropical malady, supersede those methods already in common use. In the milder forms of these diseases, and in such as have been early recognized and properly treated from the beginning, the modes of inunction and internal exhibition, have long been found to be quite commensurate to their removal; and, therefore, any attempt to change the treatment of such, would be more in the spirit of idle innovation than of real improvement. But it is matter of too frequent complaint, that, in the more violent cases of hepatitis, of the endemic fevers of India, and of the yellow fever of the western hemisphere, the rapidity of the disease does very often outstrip that of the medicine employed in its cure; and that the miserable patient is hurried away before the remedy has had time to exert its salutary influence in his favour. To the progress and cure of the maladies we are speaking of, the words of the poet admit of a very just and striking accommodation:

> ——————— horae
> "Momento cita mors venit, aut laeta victoria."

To the more violent and concentrated forms of tropical disease, and to those even of a slighter nature, but which have been neglected or improperly treated at their first invasion, I would, therefore, restrict the use of the corrosive sublimate. And in such, if the method proposed be actually found upon trial to saturate the system with as much rapidity and safety as it did in the case of Wilson, its very extensive application, and the vast benefit which will accrue from it, in the cause of suffering humanity, require not to be formally exposed. It may, finally, be added, likewise, that when the ptyalism has once been fairly established by this plan, the continued use of it may be wholly abandoned, and the mercurial action prolonged, if that be necessary, by employing the blue ointment or the submuriate.

Note.—I have seen Mr Sheppard's paper, which is published in the 52d number of this Journal, on the mercurial treatment of yellow fever; and should his conclusions be legitimate, they are of course fatal to this, as well as to every other, scheme of superinducing the action of mercury during the presence of violent fever; but the evidence of Mr Johnstone and other practitioners, who have long practised between the tropics, is so decided and strong on the affirmative side of this controversy, that one, even without pretensions to personal experience on the subject, may be permitted, in the mean time, to bring forward any hint which carries along with it as much plausibility as to induce him seriously to believe that *possibly it may be useful* to those whose duty it is to combat these formidable distempers.

Old Kirkpatrick, 7th September 1818.

V.

Case of Hydatids. By G. M.

Some time in the summer of the year 1808, when Mrs A. H. was emptying a large vessel full of whey over the door of a pig-stye, her feet slipped, and she fell across on the door with the vessel still in her hands, whereby her right side was much bruised, but she continued to go about her family business for some days without much difficulty or pain. On complaining much of the pain of her right side, Dr C. was called, who caused her to be blooded, and pursued for a considerable time the antiphlogistic treatment with vigour; general and topical bleeding, with blisters, of which nineteen were applied in succession. By a steady adherence to this plan, the pain was moderated, but there remained a chronic disease, with induration and enlargement of the organs near where the injury was received. In the epigastric and right and left hypochondriæ, this enlargement obviously protruded the abdomen, attended with pain occasionally and disorders in the primæ viæ.

Mercury was twice administered whilst in this state, without any evident benefit. For eight years afterwards, she continued to attend to her avocations. At times when the pains were acute, she got blooded, and took laxative medicines, which relieved her for the present. Still the induration and enlargement of the abdomen continued.

In May 1817, the pain in the epigastric became acute, extending along the region of the liver to the right side. A number of leeches were applied, without alleviating in the least the pain. After a few days vomiting supervened, and hydatids as large as walnuts were thrown up; a greater number and of a larger size were expelled by stool. These hydatids continued to be discharged occasionally for three weeks, when they stopped. The bowels were kept open by small doses of sulphate of magnesia, and, to alleviate the pain, an anodyne glyster was administered at bed time. She went on thus, complaining of pain near to the umbilicus, and much increased on pressure, until the end of July, when an appearance of pointing was discovered, to which an emollient poultice was applied, and continued for some days, when, on the 3d August, the tumour spontaneously opened about two inches below the umbilicus, and discharged a large quantity of sanies of a very disagreeable fetor. Prior to the opening of the tumour, the pulse was upwards of 100, but almost immediately after it fell to 80, and kept about that number until near her dissolution. About this time, there was great debility, œdema of the extremities, all kinds of aliment rejected, occasional vomiting and frequent stools, urine natural, and usual in quantity.

Nearly in this state, she continued until the 3d October, when, being completely exhausted, she died. She had been unable to take any nutriment since the tumour opened; the discharge continuing alternately to stop and flow; bowels uniformly loose, and stools very fetid.

Twelve hours after death, the body was examined, when adhesions were observed to have taken place of the viscera to the peritoneum; the hypochondriac and epigastric regions presenting such a mass of incongruous matter, that the organs and their relative connections were with difficulty distinguished.

The spleen exhibited the most prominent appearance, having acquired a size considerably larger than the human head, and extending from the left side forwards over the stomach pressing it backwards, and the liver upwards and inwards. Externally, it was like the liver in its natural state; internally, it had the appearance of the surface of a sore secreting ill-conditioned pus. It formed a huge sac containing hydatids, some of them four inches in diameter. The larger ones being burst, it was impossible to ascertain their dimensions.

The liver was not much enlarged, but presented such a mass of disease as precluded the possibility of discriminating its different departments. Both the large lobes were filled with fetid matter of a dark colour; the lobulus spigelli formed a sac containing a brownish fluid; no gall-bladder was discovered; the right lobe of the liver and intestines were adhering to the peritoneum, lining the parietes of the abdomen, on the fore part of the right hypochondria.

The omentum was obliterated or absorbed in the mass of adhesions.

The stomach, intestines, kidneys, uterus, and their appendages, appeared in a healthy state.

A considerable quantity of fluid was found in the general cavity of the abdomen.

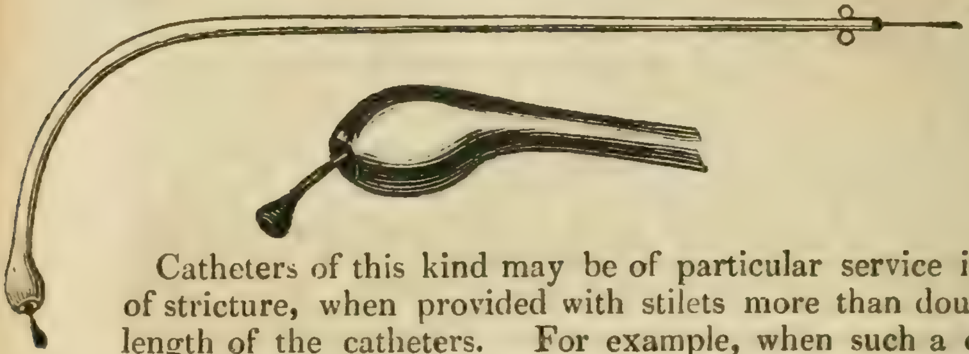
If these facts serve in the least to elucidate the phenomenon of hydatids, or open any farther the pathology of diseases in which they are found, it is gaining a little; if not, it only adds one more to the repository of medical facts, which cannot be too abundantly supplied.

Ayr, 5th October 1817.

VI.

Observations on the Catheter. By Joseph M'Sweeny, M. D.

There is nothing that should engage attention more than the means of avoiding unnecessary pain of every kind. When a surgeon endeavours to alleviate the sufferings of a patient, however trifling they may be, he advances nearer to obey the dictates of humanity, and to rescue his profession from the taunts of prejudice. The introduction of the catheter, from its frequent occurrence, claims particular attention; and, although the pain of the operation in ordinary cases is trifling, yet he who tries to lessen even this, does but his duty. In cases of great irritability of the urethra, to avoid irritation as much as possible is of the greatest importance. Surgeons of experience have acknowledged the facility of introducing a large catheter, and the reason is obvious, the point of the instrument is not apt to catch in the lacunae of the urethra. On a moment's consideration, however, a person must perceive that the rest of a large catheter, when in proportion with its point, must cause irritation without any equivalent advantage, by keeping the entire track of a diseased urethra much distended. The apertures in catheters next demand attention. There are two kinds of catheters to be found in the shops. In one kind there is a large perforation in one or both sides of the instrument, in the other, there are a number of small perforations in both sides. When a catheter of the latter description is rubbed between the fingers, a sensible degree of roughness is felt. This roughness, although scarcely productive of pain in a healthy urethra, is far different in a dry and inflamed state of the passage. The power which the urethra is well known to possess, of firmly grasping a catheter, will make it insinuate itself into any depression of the instrument. This particularly applies to the catheter that has a large perforation on one or both sides. When the urethra firmly grasps this kind of catheter, the lining membrane is pushed through the aperture into the calibre of the instrument, and is an impediment to its progress. The sharp edges of the aperture irritate the part of the urethra that has been pushed in, and cause that sensation of being cut, which patients feel. It is therefore an object to have catheters perfectly smooth, and to have a large bulbous extremity, which will facilitate the introduction of the instrument, without keeping the urethra unnecessarily distended. The large bulbous part will be in the bladder, and in cases where it will be necessary to wear this kind of catheter, the bulbous part will be an impediment to its slipping out. I had catheters of the following description made last winter in Edinburgh, of various curvatures and degrees of size. The catheter is twelve inches long, it has a bulbous extremity, and the rest of it is small in proportion to the bulb. There is a pyriform depression at the extremity of the bulb, in which depression the perforation is. The stilet is about an inch or more longer than the catheter, and has a pyriform knob at one extremity to correspond exactly with the depression at the end of the bulb. The other end of the stilet is not furnished with a ring. The stilet is put into the catheter at the bulbous extremity, and the pyriform knob fits the depression so exactly, that the catheter appears to have no orifice for the urine to escape. The stilet being about an inch longer than the catheter, it can be pushed up when the catheter is introduced. The act of pushing up the stilet will make the pyriform knob at its extremity rise out of the depression in the catheter, and the urine will flow freely out. The full flow of it may be suddenly stopped by pulling down the stilet to its former situation. In cases of calculi, the act of pushing up the stilet may be of service in keeping away concretions from the neck of the bladder when the urine is flowing.



Catheters of this kind may be of particular service in cases of stricture, when provided with stilets more than double the length of the catheters. For example, when such a catheter is passed down to a stricture, and it is ascertained, that it cannot be passed farther, it is to be withdrawn over the stilet, which is to be kept in contact with the stricture. This can be easily done on account of the great length of the stilet. Small catheters are then to be successively carried along the stilet to try if they would pass the stricture. If small silver catheters will not pass, then a very minute elastic gum one, with its orifice at the extremity, may be tried in the same manner. By this contrivance, there is no fear that the point of a small catheter will catch in the folds of the urethra, as it will be guided down to the stricture by the stilet. If the pyriform knob at the end of the stilet can be made to pass the stricture, all fear of using a conical silver catheter, with an orifice at its extremity, is at an end. It can be carried down the stilet in the same manner, and can be forced through the middle of the stricture without the fear of pushing any of the urethra into a pouch. The passage of this conical catheter through the stricture, and its arrival at the end of the stilet, may be ascertained by the percussion against the pyriform knob. This conical catheter should not be conical the entire length; the thick part would irritate the orifice of the urethra too much. I think it would be sufficient to have one conical one-third of its length only Catheters, as described in this paper, have been admirably finished at the house of Still, Infirmary Street, Edinburgh. Care should be taken to have the pyriform knob perfectly smooth at every part, that it might not cause irritation when left by itself in the urethra. If the depression at the end of the catheter be properly finished, the instrument can be introduced easily without its stilet, Large catheters of this kind without their stilets may be worn by calculous patients, in cases where urinary concretions are passed. If a small concretion should get in at the orifice of such a catheter, the patient will be saved the agony that would attend its passage down the entire track of the urethra.

Paris, October 2, 1818.

VII.

Fever, sometimes the Effect of Depletion, by Bleeding, whether accidental, or employed for the Cure of Disease. By NATHANIEL RUMSEY, Member of the Royal College of Surgeons of London, and Surgeon at Beaconsfield.

THE following facts have led me to the opinion, that loss of blood, either when taken away for the cure of disease or occurring by accident, in some instances produces fever; and depletion, by bleeding, being an important remedy in constant use for the cure of the most serious diseases, it will readily appear, if I am right, that it may sometimes so modify or distort the natural character of disease, as to lead the practitioner to an erroneous conception of the state of his patient, and a wrong use of remedy. Hence the importance of pointing it out.

Whether the facts justify such a conclusion, I beg the favour of submitting to the public, through your valuable Journal.

CASE I.—Upwards of two years ago, I was called hastily to a gentleman, who, falling down with a bottle in his hand, divided the palmar artery, which bled with great violence. He was about sixty years of age, of a robust constitution, and sanguineous temperament; florid complexion, and rarely subject to indisposition. The bleeding had deprived him of colour, and his appearance betrayed an approach towards syncope. I bound up the wound; and although I ultimately took up the ulnar artery in consequence of returning hæmorrhage, several days elapsed, with no circumstance to influence health, but the loss of blood at the moment of accident. Before it had happened 24 hours, my attention was arrested by observing him under a most distinct paroxysm of fever, or such symptomatic irritation as local inflammation or other disease is continually seen to produce. The heat of skin, fur on the tongue, quick and full pulse, were such as might have induced me to recommend venesection in the strongest way, had not their origin been obviously one which could not require it.

It was the first time I had noticed the production of fever by loss of blood, simply, and I thought it worthy of recollection.

Case II.—In the autumn of last year, (1817,) a young woman of delicate appearance, with fair complexion and red hair, called to inform me that she had laboured under a hoarseness for some weeks, but had continued, without interruption or illness, to fulfil the duties of a maid-servant. I could discover no symptoms of general indisposition, and hesitated whether to prescribe tonics or depletion. I determined upon the former, and after two weeks use of them, finding no amendment, and hearing that she had failed to mention a discharge of blood by coughing, which was the first symptom of her attack, I thought it right to open a vein. The blood flowed freely, and she soon became faint. In my frequent visits up to this time she had kept about. I had never found her skin hot, or suspected that she had laboured under any of the symptoms of fever; but on the following day I found her unable to sit up, her skin being hot; pulse strong, full, and frequent, with a furred tongue; urine scantily secreted, and depositing sediment. This febrile state continued gradually abating each day, till in about a week it was almost gone. Though the hoarseness has continued three months since, with but little amendment, there has been a freedom from every thing like this distinctly marked fever. The heat of skin, quick pulse, and morbid secretion of urine, are gone. From the most careful inquiries I was able to make, I could discover no cause for the sudden accession of fever, except the bleeding *ad deliquium*, which I fully believe had produced it.

Case III.—Master H. a fine boy about thirteen years old, of animated disposition and pale complexion, after a few days of slight indisposition became clearly affected with peripneumony. I took away seven ounces of blood, which, after standing, exhibited slight marks of inflammation. Syncope was not produced. A slight tendency to nausea came on and lasted a few seconds. The local symptoms were diminished. After the lapse of 30 hours, (the marks of debility, which the bleeding had produced, having soon disappeared,) an increase of local pain and difficult respiration returned, with hacking cough, quick, full, and hard pulse. I bled him *ad deliquium* while recumbent. Immediate abatement of the local effects of inflammation followed. At the end of 14 hours from this period, I found him relieved of pain, capable of respiring fully, with much less tendency to cough. After making the inquiries, which gave me this satisfactory information, I took him by the hand, and to my great surprise found it very hot; his pulse 125 and full, though not hard. A little brown fur adhered to one of his front teeth, and his fever appeared so out of proportion to the apparent state of the local disease, that I felt apprehensions that his case might be combined with typhus fever. This febrile state continued through the day, and for the satisfaction of his friends, I permitted the application of a few leeches. They bled freely, but produced no deliquium. On the following morning I found him less feverish, yet his pulse still 120; local symptoms nearly gone. No return of local symptoms took place; his fever disappeared within a day or two, and convalescence was rapid.

This last case, perhaps, exhibits the fact I endeavour to illustrate in a less striking way to the reader than the preceding ones, although not less forcibly to my observation; and I chose it, not so much for the purpose of conveying my opinion, as to point out my belief that fever may be produced by the aforementioned cause, as well in cases whose character consists partly in febrile irritation, as in those in which there was previously none.

It is scarcely necessary to state, that this effect of depletion is far from constant; but I think it consistent with candour to affirm, that, with my attention directed to the subject, I have lately met with several instances, in which no fever seemed to be produced by venesection largely employed.

That the paroxysm of fever arose from loss of blood in the first case, I think can scarcely be questioned. No remarkable agitation of mind took place at the time, and, if it had, any effects upon the system arising from it, I suspect, would have been immediate, and not of the nature of fever, so distinct, and at the distance of many hours. With regard to the second case, prepossessed with a belief that depletion was capable of producing fever, and seeing a patient suffering under a disease which produced none, submit to depletion, and immediately fall ill with feverish irritation of a few days only, while the original disease survived it, unabated, and exempt from it, leaves me but little room for a doubt upon the subject, especially as, upon strict inquiry, no other cause appeared. The third case exhibits the fact more ambiguously. That the local disease and feverish irritation were disproportioned, would seem to argue but little, since the many varieties to which peculiarity of constitution and circumstance give rise, preclude the thought of fixing a given degree of fever to its proper cause; but in this instance the disparity was great, the symptoms of local disease had become perceptible to observation and inquiry, and the speedy and perfect recovery shewed that there was in fact a greatly diminished pulmonic disease at the moment that febrile irritation was at its greatest height.

I regard it as a professional duty to communicate the above opinion, because it is obviously important to discriminate, when fever exists, whether it arise from local inflammation or from loss of blood; for in local inflammations, the practitioner is jealous of renewals of disease, and much disposed to consider febrile action as one of the strongest characters of it; yet, if my observations are correct, febrile action may sometimes be the signal that depletion should proceed no farther, and not at all the proof of a remaining necessity for it.

I have met with several other cases which have tended to confirm my opinion, which, if it were necessary, I am not prepared to detail.

VIII.

Cases of the Successful Treatment of Scarlatina. By ALEXANDER TWEEDIE, M. D. Fellow of the Royal College of Surgeons of Edinburgh, and one of the Surgeons of the Edinburgh New Town Dispensary.

THE attention of the medical profession has been lately excited by a valuable paper by my friend Dr Abercrombie, on the effects of blood-letting in a certain description of dropsical affections.*

Although the practice had been adopted by the ancients, and attracted the notice of some continental authors, still, in this country, the attention of the profession had not been called to the subject till Dr Abercrombie pointed out, with his usual accuracy and perspicuity, the precise description of cases to which this mode of practice is applicable. From the success with which the cases thus treated has been followed, we shall now be much less inclined to dread the use of the lancet in dropsical affections, connected with inflammatory action in any of the cavities.

The benefit which resulted from venesection in the affections above alluded to, has led to its adoption in dropsy from other causes, or as a sequela of other diseases.

The dropsical effusion which follows scarlatina has, in many instances, been found to resist the treatment by purgatives and diuretics. This has also been pointed out in the above paper; and more lately a very important case under the charge of Dr Gairdner has been described in No. 55 of your most valuable Journal, illustrative of the decided efficacy of this practice. As this is a practical point of some importance, I shall offer no apology for giving the detail of three cases which have lately come under my observation.

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* Edinburgh Medical and Surgical Journal, No. 54.

J. M. æt. 5 years, applied at the New Town Dispensary on the 30th September last. She was affected with general anasarca, more particularly of the face and upper extremities; frequent short cough, with hurried and oppressed breathing. The abdomen was tumid, and the lower extremities were œdematous. When she was placed in the horizontal posture, the dyspnœa and cough were instantly increased, so that her nights were passed with great uneasiness. Urine was scanty; pulse 130; skin hot, with furred tongue. She had passed through a mild attack of scarlatina about ten days previous to the occurrence of the above symptoms, but had laboured under hooping cough in a mild form for nine weeks. A purge of calomel and jalap was ordered by Mr Ortt, pupil of the Dispensary, which operated well.

Oct. 1.—No alleviation of the symptoms. The purgative was ordered to be repeated.

2d.—Free alvine evacuation; breathing appears somewhat relieved, but still oppressed, with frequent cough; no increase of urine; pulse 126.

Emittantur e brachio sanguinis ℥iv. vel v.

3d.—Blood slightly buffy; cough less frequent; breathing much relieved; anasarca considerably diminished; urine increased in quantity; has passed a better night, and appears active and lively.

4th.—Anasarca still further diminished; cough nearly gone, and breathing natural; urine abundant.

5th.—No complaint. Convalescent.

R. B. æt. 5 years, eight days after a mild attack of scarlatina, through which he passed without any untoward symptom, became affected with general anasarca, short dry cough, oppressed breathing, and scanty secretion of urine; pulse 144, sharp.

He took a brisk laxative for two successive days, which operated freely; but the cough, oppressed breathing, and scarcity of urine, continued.

On the 3d October he was bled to the extent of ℥v. with immediate and decided relief of the pectoral symptoms.

4th.—Cough and dyspnœa still further relieved; anasarca much diminished; urine more abundant; blood not sizy; pulse 120.

5th.—Anasarca almost gone; pectoral symptoms continue moderate; has passed a quiet night; urine copious.

6th.—Free from complaint.

M. D. æt. 15 years. About six weeks ago, she laboured under scarlatina. After the disappearance of the efflorescence she became affected with anasarca, which first appeared on the face, and gradually extended generally on the trunk and extremities. The abdomen was so much distended, that, to use her mother's expression, "her corsets did not meet by at least six inches." She at the same time complained of pain in the left side of the chest, with dyspnœa and cough, accompanied with sensation of suffocation when she assumed the horizontal posture. These symptoms had existed for eight days previous to her application at the New Town Dispensary.

Oct. 12th.—Ft. venesectio ad ℥xii.

13th.—About ℥x. drawn; after which she felt considerable relief of the pectoral symptoms; appearance of the blood not ascertained; has passed a good night; anasarca diminished.

Hab. pulv. purgant. statim.

14th.—Cough and oppressed breathing nearly gone; anasarca has still further subsided; bowels open.

16th.—No return of pectoral ailment; anasarcous swelling quite gone; urine passed in abundant quantity; makes no complaint.

18th.—Convalescent.

Since the above cases came under my observation, two brothers of J. M. have had an attack of scarlatina; which was followed by dropsical effusion, accompanied with pectoral symptoms. It is unnecessary to detail minutely the progress of the disease, as the symptoms were precisely similar to the case alluded to.

In the one (a boy of six years of age) in whom the symptoms were urgent, bleeding from the arm to the extent of five ounces completely removed the pectoral affection, which was followed by the speedy disappearance of the dropsical effusion. In the other, a child of three years of age, the symptoms yielded to the repeated application of leeches.

IX.

A short Account of Cubebs as a Remedy for Gonorrhœa. By JAMES ADAMS, Member of the Royal College of Surgeons, London; Surgeon late 4th Ceylon Regiment; and late Garrison Surgeon, Point de Galle, Ceylon.

IN the month of November last I was favoured with a few ounces of cubebs, by a respectable medical man in Dublin; together with the following account of the plant, and of the mode of administering it in the treatment of gonorrhœa.

"This spice is the produce of the Island of Java, and is also found in many parts of China, and has within the last three years been introduced in the hospitals by order of the Medical Board at Bengal, as a specific for gonorrhœa Dose,—one dessert spoonful of the powdered cubebs an hour before breakfast; a second at six hours; and a third when going to bed.

"The powder to be taken in water. If taken in a case of violent and recent infection, where the discharge is considerable, and the inflammation and scalding great, the painful symptoms will be removed in two days, and the discharge generally terminates on the third or fourth day.

"The antiphlogistic regimen is to be attended to, and it is also necessary to continue the powder a day or two after the discharge has disappeared. In selecting cubebs it is necessary to be very particular in choosing it with a stalk attached to it, as cubebs so much resembles the common black pepper, that some is very frequently mixed with it. Almost any quantity of this spice may be taken in water without the slightest inconvenience."

The following case of gonorrhœa was treated with fresh cubebs, direct from Java. The spice was reduced to a fine powder, had an aromatic taste, with the flavour of ground pepper, and when rubbed between the fingers had an oily feel; its colour a dark-brown.

December 1st, 1817.—Mr ———, aged 23, has a discharge from the urethra of five days standing. No pain unless when passing his urine, which is attended with a considerable degree of scalding. Has not made use of any remedy. General health and appetite good.

℞ Pulveris cubebæ drachmas duas.
Aquæ puræ uncias quatuor. M. fiat haustus ter in die sumendus.

December 2d.—Discharge as yesterday. Says his scalding is considerably better.

Repetantur haustus tres.

December 3d.—A slight discharge from the urethra. No pain in passing his urine.

Repetantur haustus tres.

December 4th.—Discharge and scalding removed.

Repetantur haustus tres.

December 5th.—No return of symptoms.

Repetantur haustus tres.

December 6th.—Says he is perfectly well.

In this case the cubebs agreed with the patient's stomach, acting gently on his bowels, and in the course of one day afforded considerable relief from the most troublesome symptom. If necessary, I could add a number of cases in which I have directed cubebs almost with equal success; at the same time I must candidly state, that I have met with a few that* resisted this remedy, and when we consider Mr John Hunter's opinion of the efficacy of medicine in the treatment of gonorrhœa, we shall not be surprised at occasionally meeting with a case that will not yield to the use of cubebs. Such I believe there are, and may depend on the parts being very susceptible of such irritation and tenacious of it.

In the course of last month I had an opportunity of conversing with an army medical officer who served with his regiment on the island of Java for the space of two years. That gentleman spoke in the highest terms of the efficacy of cubebs in the treatment of this complaint. He had long been in the habit of using it in his hospital, and he assured me he seldom met a case that resisted the free use of this spice, when fresh prepared and finely powdered. He attributes much of its efficacy to the essential oil it contains.

From the foregoing observations, and from the best information I could procure from my medical friends in this country, together with my own experience of the efficacy of this remedy in the treatment of gonorrhœa, I am induced to offer the following conclusions, viz. Cubebs is not a specific for gonorrhœa. At the same time, it may be considered a valuable remedy when of a good quality, fresh, and finely powdered; and, if I mistake not, it is deserving of as much reputation as any other remedy now in use for the cure of that disease.

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* In referring to my case-book I find those patients who did not derive any benefit from the cubebs, had previously used other remedies for the space of eight or ten days; their inflammatory symptoms had in a great measure subsided, and the disease had so established itself, as not to be under the control of so simple a remedy.

From such considerations, I am induced to offer the foregoing remarks to my contemporaries in the profession, together with a most anxious wish, that some of our eminent practitioners in surgery may be induced to consider the subject, and give to the public the result of their observations.

Rathmines, August 30th 1818.

X.

On the Present State of Vaccination. By Mr THOMAS BROWN, Surgeon, Musselburgh.

SIR,—I intended long before this to have returned you my best thanks, for the very handsome manner in which you have retracted your opinions concerning the sentiments I entertained of the Jennerian practice, and to have endeavoured to give a satisfactory answer to the different queries, which you very properly put, with a view of coming to some understanding upon the subject; but I am sorry to find my professional avocations will not allow me to follow the plan I proposed, and I must content myself with presenting you a brief summary of my present opinions on this important subject.

I must, however, Sir, first take the liberty of observing, that no one could feel more deeply the disappointment of vaccination being found inadequate to give perfect security against small-pox than I did. I was the third practitioner in Scotland who was favoured with virus from the late Dr Anderson of Leith; and, from a single lancet, I prosecuted the practice to the fiftieth series; and, in less than two months, from my first use of the virus, the small-pox inoculation was entirely superseded in my practice; and before I considered it my duty to lay my doubts of the efficacy of the vaccine practice before the public, I had vaccinated upwards of 1200 cases; a number certainly sufficient to prove that my motives could not be selfish or interested, when I was declaring, not only that the opinions I had given, and the sentiments I had entertained for nine years, were erroneous, but that I had received remuneration for a practice, which I now declared to be almost useless.

It was a few months after I had begun the general use of the vaccine inoculation, that I had occasion to meet the late Dr Alexander Monro; and, upon my informing him of my having adopted the new practice, he made the declaration, which I have noted in my dedication to him, when I first submitted my sentiments on this subject to the public; viz. "Such was his opinion of the efficacy of the practice, and the distressing consequences likely to result from it, that it ought to be prohibited by act of Parliament." I frankly confess, I considered the Doctor's opinion as being merely the result of reasoning from analogy, and not entitled to any weight; in opposition to the two important facts, which experience then afforded in favour of the Jennerian discovery, viz. that if you submitted the vaccinated cases to the tests of inoculation with variolous virus, or exposure to the epidemic, they resisted both. Notwithstanding such important facts, however, I am afraid it will turn out true, that it would have been well for the world, had the Doctor and other eminent characters maintained their want of confidence, and not have allowed these two facts, however striking, to overturn what had been the result of long experience; but he and every man of any eminence in the profession, not only rapidly became converts, but unfortunately, considered any further inquiry and proof altogether superfluous, and all became *parties* to the success of the Jennerian discovery.

I can assure you, Sir, none of the medical profession, be he who he may, could outstrip me in zeal for promoting the vaccine practice, until the summer of 1808, at which time the facts occurred, which I thought myself not warranted in resisting, and considered it my duty to lay them before the public. I must now be allowed to declare, that I feel much pleasure in finding my observations were well founded, and that the sentiments and opinions they suggested are likely to be confirmed. There is surely, Sir, neither less merit, nor less virtue, in correcting a public error, than in propagating a public good; and I hope those gentlemen who took so much liberty in ascribing such improper motives for my conduct, will follow the liberal example you have shown them, and acknowledge that I have been an attentive and accurate observer.

When I determined, Sir, to publish my sentiments upon this subject, I was fully aware of the contemptible state of those who had hitherto opposed vaccination, and I shuddered at the danger of being classed amongst them, and of opposing nearly the whole of the respectable part of the profession and community; but the proof which I met with, appeared to me so clear and decisive,—corresponded with a minute examination of even Dr Jenner's own statement,—and was supported with all the observations I had made, relating to the inoculation of small-pox, together with my experience of the other exanthemata,—I determined to risk every species of detraction, and do what appeared to me my duty; and I think, Sir, you will readily acknowledge, neither credit nor honour can result from discerning and acknowledging facts, only when they shall possess perspicuity sufficient to make an impression on the faculties of an idiot, or force sufficient to constrain the resistance of madmen.

Upon considering the sentiments I have thought it my duty to publish, in various ways, upon this subject, I find I have really little to add, and that nothing is left for me to do, but to give a summary of what appears to me to be the present state of the vaccine practice.

The small-pox have been known in this island as an epidemic, for nearly a thousand years, and, upon an average, one person in five fell a victim to the disease; during which period, the universal opinion was, that whoever had once undergone the disease, in a *distinct* form, never suffered a second attack.

It is now nearly a century, since the population of Great Britain began to place their security against the natural small-pox, by having recourse to the practice of inoculation; and all those who chose to avail themselves of that practice, escaped the desperate chance of one in five against their existence, at only the hazard of one in five hundred; and it was allowed on all hands, that wherever a *distinct disease* was produced, the same security was imparted from that process, as was derived from the epidemic disease,—perfect satisfaction was obtained, and all future anxiety and apprehension were destroyed.

The circumstances which rendered us doubtful of the future security obtained from undergoing the small-pox, either from the epidemic contagion, or from inoculation, were, when the fever was not severe, and a very scanty eruption of ill defined pustules took place. And, notwithstanding such occasional deviations from distinct disease, still the instances of a second attack of small-pox were so rare, as to make no exception to the general rule, and even excited the strongest doubts of the correctness and reality of such cases, in the minds of the most respectable part of the profession.

Our experience, with regard to any other disease which the general consent of mankind allows only to occur once in our lives, is exactly the same, viz. they are only known to exist from the uniformity and distinctness of their characteristic symptoms; and, if these are awanting, we have our doubts either of their existence, or of the ultimate protection they will afford.

Our experience also taught us, that the human body, while either labouring under some diseases, or for some time after they were over, was not so liable to be affected with the influence of other diseases; while others, again, afforded no protection, or rather offered no resistance whatever. Our experience afforded no proof, that the influence of any of the exanthemata extended their security, but against their own peculiar disease.

When the Jennerian practice was introduced to public notice, the whole medical profession entertained great doubt of its powers in being able to confer, either immediate or future security against small-pox, chiefly upon two grounds, viz. that of possessing no character resembling the severe disease it was meant to combat,—and of exerting no sensible or distinct influence upon the human body.

It was from the two circumstances of resisting exposure to the epidemic and inoculated contagion, the impression was produced upon the medical profession, that the vaccine process might be trusted to, as giving security against the contagion of small-pox; and these two tests were applied to every vaccinated case upon the first introduction of the practice, to the extent of more than 100,000 cases, and all were uniformly found to resist these tests.

If we examine the phenomena produced by the vaccine inoculation, we shall find they consist almost entirely in the appearances of the punctured point, at all times without any *distinct* febrile affection, and only occasionally with any sensible derangement of the system: That these phenomena admit of a prodigious variety, over which the practitioner, be he who he may, has no control, and still exert the same influence over the human body, in enabling it to resist the two tests already mentioned.

It is also ascertained by experiment, if you introduce small-pox and cow-pock matter at the same time, by separate punctures, and the usual local phenomena proceed exactly together, still the disease produced will be small-pox. Again, if you introduce the cow-pock virus two or three days before the small-pox matter, still the disease will be small-pox, but somewhat modified; and it requires the vaccine inoculation to be performed eight days previous to the introduction of small-pox matter, in order to anticipate entirely its effects upon the system.

Experience has also shewn, that the natural small-pox have made their appearance, when the vaccine puncture had previously existed, surrounded with its areola of the most perfect appearance for more than two days, and not in the least modified, but in the highest degree confluent, and followed by death. Small-pox pustules, too, existed within the very areola of the vaccine puncture.

Experience and experiment have also shewn, that, upon having recourse to exposure to the epidemic small-pox some years after having undergone vaccination, the individual is found to be influenced more or less by the contagion, and is liable to the disease of small-pox; and if inoculation with small-pox is again tried at the interval of some years from vaccination, not only the perfect local phenomena of small-pox inoculation will be obtained, but also fever and eruption; and these two tests, will now, in general, be found to exert their influence upon the constitution, exactly in conformity with the distance of the period from vaccination.

In conformity with this observation, experience has shewn, that for the first two or three years after the introduction of vaccination in Great Britain, scarcely a case of failure was heard of; but afterwards, they have been gradually increasing in number and severity every successive year; and that the number of such cases of failure have been met with in the respective situations, exactly in proportion to the early introduction of the Jennerian practice,—the extent of population, and the proportion of the lower classes of the community.

The accounts from all quarters of the world, wherever vaccination has been introduced, are assuming exactly the same aspect; and at the present period it cannot be disputed, that the cases of failure are now increased to an alarming proportion; and from a fair and impartial examination it appears, where the small-pox contagion has access to operate upon vaccinated cases of upwards of six years standing, and the contagion applied in a concentrated and lasting form, nearly the whole of such cases will yield to the influence of the small-pox contagion.

It therefore appears, it is incorrect to say, a very large proportion have resisted the influence of the small-pox; it would be more consistent with truth to state, "that they have not hitherto taken the small-pox;" because it is necessary to shew, such proportion have been distinctly exposed to a satisfactory form of the epidemic, and that a proper period has elapsed since they have undergone the vaccine disease. Besides, it is to be remembered, there are many instances, where individuals resist the contagion at one time, and are subject to its influence at another; and also, there are not a few constitutions which are susceptible of neither small-pox nor cow-pock.

But it is not only in the number of failures that this subject becomes alarming, it is also in the severity of the disease; and at this period, many of the instances of failure not only assume the most severe form of the distinct disease, but also have been confluent; and death has followed in a considerable number of cases.

Experience has shewn, that the cases of failure, as well as the mildness or severity of them, do not always correspond with the proportion of their distance from vaccination; but these will be found satisfactorily explained, either from the severity and mode of application of the small-pox contagion, or from the different extent of influence imparted to the constitution, by the variety in the vaccine phenomena.

It appears, that neither this unfortunate imperfection of the vaccine process in giving security against small-pox contagion, nor of the variety in the phenomena which the process may undergo, can admit of any remedy. The vaccinations conducted by Dr Jenner, by public institutions, by private practitioners, by ministers, midwives, or farriers, all have failed: whether the process has been conducted by one puncture, by two, or even by four; or whether the eventful test has been applied; * all have fallen short of the desired effect. The truth is, the only difference of the modes of conducting the vaccine process rests merely in extending or diminishing the period of security *a year or two*, and but of little consequence exactly to ascertain. There, however, can be no doubt, that, according to the extent and severity of the phenomena which attend the puncture, so will the impression be upon the system.

If, in order to avoid these consequences, you select the virus uniformly of the same limpid quality—use a lancet of the same edge—introduce it in the same manner,—and treat the arm afterwards in the same way—still a great variety will follow, and which no human being can prevent, but which will be found to admit of very little difference in the length to which the period of security extends. But if the virus shall be taken viscid, weak, or in the state of scab;—if it shall be introduced by puncture with a very fine edged lancet;—if the puncture shall be slight, and if only one,—then, in general, the operation will either fail, or a very imperfect vesicle will be produced, and of course a very feeble impression made upon the system, and which will uniformly allow the operation of the small-pox contagion to influence the system some years earlier, and produce a more severe disease.

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* Mr Bryce's method of vaccinating with the test puncture.

The unfortunate consequence of all this, however, is, that although you may be sensible the vaccine phenomena have been imperfect, and not so satisfactory as you could have wished, you cannot remedy it. The very feeblest and most unsatisfactory phenomena make such an impression upon the system, as will be found, either to prevent you obtaining any farther satisfaction by *revaccination;* and if you should inoculate, or expose them to the small-pox, they will be found to resist at that period both these tests.

I am decidedly of opinion, there is no mode of giving the vaccine disease by inoculation, which can impart *perfect and permanent security* against small-pox contagion; and I apprehend it is impossible to give the disease in any other form, to obtain that effect. For, if any one will attentively examine the whole history of vaccination, from Dr Jenner's publications downwards, he will find the proof of its powers to impart *permanent* security, almost gratuitous. But even allowing that it were possible to obtain perfect security, by receiving the disease from the cow, I apprehend no one will ever contend the practice can be carried into effect, if we are under the necessity of becoming milkers in the county of Gloucester. Indeed, Dr Jenner's cases do not amount to thirty, where he stated such exemption had reached to the extent of 30 years, or for life; and certainly there is no improbability, or impossibility, in supposing such number, either to be kept in a state of security, from continuing to exercise the office of milker, or to be those peculiarities of constitution, not susceptible of small-pox; which number is certainly not too many to exist in the populous county of Gloucester.

The whole phenomena and history of vaccination, together with those of variola, rubeola, scarlatina, &c. &c., and even the action of mercury, shew,—a specific action may not only exist minus in the system, but even in various degrees, and that in proportion to the extent of their action, so may the permanency of their influence be estimated. Therefore, as the process of vaccination evidently exerts only a feeble influence over the human body, the extent and permanence of its effects are to be estimated accordingly.

I think it is also perfectly clear, and which ought to be considered as decisive, that, while we continue to maintain the antivariolous power of vaccination is perfect and permanent,—contradiction, confusion, and obscurity attend the subject; but the moment a partial, feeble, and temporary influence is admitted, no contradiction of facts exist, and harmony pervades the whole subject.

I shall now, Sir, proceed to examine more particularly, the reasons which are stil given by the friends of vaccination, for persevering in placing our confidence in the Jennerian discovery; and I must be allowed to premise, once for all, that, after the various tergiversations of these gentlemen, it is impossible to allow much, if any credit, to the different opinions and defences they now bring forward. As experience has taught us, what they formerly maintained as facts, must now be surrendered as no longer tenable, it is but fair to conclude, their present defence will undergo the same fate; and that, as they must feel their reputation interwoven with the issue of this question, their evidence must be considered as that of parties to the cause, and entitled to very little attention.

As the advocates of the vaccine practice cannot now deny, that cases of small-pox have occurred after the vaccine process has been conducted, even according to their own opinion, in the most satisfactory manner; they still contend the practice is entitled to full confidence, on the ground, that if so conducted, " it will *always* render any succeeding attack of small-pox mild; and there is not the most distant proof of such protection gradually wearing out, and at last becoming extinct." Also, if there should be any *severe* cases of small-pox succeeding to cases of vaccination, which they cannot challenge, they get rid of the difficulty by contending, " that there are certain peculiarities of constitution which resist the influence of the most perfect vaccine phenomena altogether."

I hold it, Sir, to be unnecessary to enter into any farther discussion, with regard to the possibility of conducting the vaccine process, so as to impart permanent security against the small-pox contagion,—referring you for a more particular examination to my publications upon this subject; and shall content myself with a short examination of Mr Bryce's method of conducting the vaccine process, by means of his test. In the *first* place, it it to be observed, the experience of every practitioner distinctly confirms the fact, that, when a circular redness diffuses itself suddenly round the punctured point, and while it remains so, the vaccine process is communicating whatever influence it is capable of exerting over the constitution; and this areola is considered by all practitioners as the proof of the *ne plus ultra* of the disease. Also, every practitioner who has attended to this subject will declare, they never yet saw an areola produced in the secondary, or test puncture, unless it previously existed in the first; and again, there is no medical gentleman of experience but will aver,—they have obtained resistance against the tests of inoculation and exposure to the small-pox, from the vaccine process, accompanied with every variety in the extent of this areola; also, the experience of all will confirm, that you may vaccinate 500 cases, and, if the disease *proceeds to a vesicle*, the areola will not be awanting *in any one of them*; and *lastly*, Dr Monro's family shew most distinctly, that when the process is even conducted by Mr Bryce, accompanied with his test, no better result is obtained. It is also to be remarked, that all our former experience in conducting the small-pox inoculation is in unison with what occurs in vaccination, and confirms the same fact most distinctly. But, Sir, the practice of the test not only possesses no advantage, but is even attended with several disadvantages, which is sufficient to cause its rejection, more especially if conducted in the mode recommended by Mr Bryce, viz. puncturing the first vesicle, about the fifth day after the virus had been introduced, in order to obtain virus for making the test puncture; by which plan you frequently injure the first vesicle so much, as to destroy and interrupt the regular progress of the disease, and you also frequently get so very little virus, that the test puncture totally fails. The test puncture may also fail from other causes, and, of course, the confidence of those concerned unnecessarily shaken.

What I have now stated, I know to be facts from my own experience and observation. Long before vaccination, I was in the habit of performing many of my inoculations in the following manner—viz. With only one infected lancet, I made two punctures; the first was the one I intended should be trusted to for giving the disease, and I bestowed the most of the virus, as well as made a more severe puncture upon introducing it, and the remainder of the matter was introduced with a slight puncture. The result frequently was, that the last puncture was full two days later in its progress than the first, and, when the first puncture assumed its areola, or, in other words, affected the constitution, the second rapidly followed. This practice I of course frequently followed in my vaccinations, and it was attended with the same consequence, and the test required was obtained without injuring the first puncture,—an object, in my opinion, of the very first consequence, and which no scarcity of virus should be allowed to disturb. But it was only when I had but a scanty supply of vaccine virus I followed this plan, as I always considered it of the very last importance, in conducting the vaccine process, to produce as severe a disease as possible; and for this purpose, I always selected virus in a limpid state,—introduced it on a lancet moderately sharp,—partly in the form of puncture and scarification combined,—in two places upon one arm, about an inch from each other. The next day, I generally ordered a stripe of flannel to be put round the arm, and kept it applied until the punctures became vesicular. I never allowed them to be disturbed in order to take virus, until the areola was beginning to form, and then only one of them. If one of the punctures failed, I never took matter from the other, until the areola was completely formed. If the extent of the areola did not satisfy me, I then used gentle violence, and pressure on the vesicles; and ordered the child to take its sleep, lying upon the same side with the vaccinated arm.

With regard to the curious excuse contrived by Mr Bryce to defeat those cases of small-pox which have occurred after his vaccinations, viz. "that, although the vaccine phenomena are all present, and exist in their most perfect form, still the constitution is not affected by the vaccine disease, and is left exposed to the influence of small-pox contagion," I really think, if we were to admit such a defence, it might be fairly concluded, we have bid an eternal adieu to the exercise of our judgment. I shall leave it, Sir, for you to determine, if it is not ridiculous and absurd in the extreme, after having described vaccination as so simple, and so uniform, and to consist of certain phenomena which were invariably found to produce such an effect upon the constitution, as to prevent any farther effects from a second vaccination; to resist small-pox inoculation, and exposure to the epidemic; and also at first nearly to annihilate the small-pox wherever the practice existed to any extent;—to come forward and maintain, merely because small-pox have succeeded to vaccination, that the process is intricate, uncertain, and difficult to conduct; that there are none of the phenomena to be depended upon as indicating the action of vaccination upon the system; that the resistance of the tests of revaccination, inoculation, or exposure to the epidemic, is not to be depended upon; that, although symptoms marking a constitutional derangement are present during the existence of the vaccine phenomena, still it is not owing to that process, *but to some diseased action of the constitution*; that the production of a small-pox pustule by inoculating those who had undergone vaccination some years before was of no consequence, even although attended with an areola and constitutional disorder, *because it is a matter of fact, and is either well known, or ought to be known, to every surgeon, that these can be produced at pleasure; and one of the gentlemen connected with the Vaccine Establishment in Edinburgh, has kept up a supply of small-pox matter for inoculation, by successively inoculating his own arm.* The scab or crust which remains after the disease was completed, which was supposed to indicate the perfect disease, is, *after the failure has taken place*, found out to be too little or too large, too light or too dark, or to have fallen off too early or too late. The cicatrix, too, is declared, if little, to prove that the local affection has been trifling and imperfect; and if large, it indicates some other diseased action. If the cicatrix is deep or superficial, the same conclusions are drawn. Nay, if it is of the very exact dimensions, still it is too much indented or serrated, to allow the process to have been complete. Here, Sir, I may surely be allowed to inquire, what length are their excuses, and the good humour and indulgence of the public, to be carried? Nothing but the most desperate and headstrong zeal can demand credit for such a tissue of absurdity; and I conceive it perfectly sufficient to enumerate them, in order to produce their refutation, and expose their folly.

The next reason given for still placing confidence in the efficacy of the vaccine inoculation, is, "that a very large proportion is still found to resist the influence of the small-pox contagion."

Those gentlemen who have stated this as a sufficient argument for persevering in the vaccine inoculation, do not even hint at what they mean by a very large proportion; but when we examine the different results which experience now affords, we shall find the most decisive evidence for rejecting any hopes which may be entertained from this quarter.

It may be first observed, that, in the certainty and consequences of variolous contagion exerting its effects upon the human body, everything depends upon the accumulation of contagious matter, and the extent, duration, and mode of application. There is not a member of our profession of any experience, but must have observed these circumstances distinctly exerting their influence in the propagation of every contagious disease, and also have met with many instances, where individuals have resisted the influence of contagion at one period, even although placed in favourable circumstances for the operation of the epidemic, and have been afterwards subjected to the disease in its most severe, and even fatal form.

I can assure you, Sir, in examining my own practice, few or none escaped at the distance of six years after vaccination, that were placed in circumstances favourable for the operation of the epidemic; very few at four years; and the greatest number who resisted the contagion, were either within four years, or not exposed to a concentrated and extensive application of the contagion; and, at the moment I am now writing, cases of failure are occurring here daily, exactly in conformity to these principles; and a few deaths have occurred, where the vaccine operation was not only performed, but the disease stated to have run its regular course.

From an attentive examination of many of the publications upon this subject, and more especially those which have appeared lately, viz. Dr Monro, Dr Dewar, Dr Thomson, Mr Hennen, and the Dispensatory, you will find these peculiarities attach to the practice of medical practitioners in general; extending to from two to four children in one family, whether vaccinated by the same, or by different practitioners, at the interval of years. Indeed I will venture to assert, if proper steps are taken to secure the application of an active contagion, three vaccinated cases out of four will be found to be influenced by the epidemic, if the vaccination exceeds six years. In some instances, I have seen families, where the whole of the vaccinated cases yielded above four years from vaccination, if I was allowed to regulate the application of the epidemic. Indeed, Sir, it was from tracing the regular progress of the contagion through the families of a whole village,—vaccinated by a great number of practitioners, in a great variety of ways, attended with an uniformity in the result, which it was impossible could be the effect of accident,—the conviction was firmly established in my mind, that the vaccine influence upon the human body, as enabling it to resist small-pox contagion, was feeble, partial, and temporary.

I cannot close this part of our subject without noticing a fact which has been triumphantly, but, in my opinion, very injudiciously and imprudently employed, by the supporters of the vaccine inoculation, for recommending this practice to public confidence,—I mean, Sir, that cases of failure have been much more numerous amongst the lower than the higher classes of society. Surely, Sir, it is madness itself to contend we have been so base and wicked as to have vaccinated the rich perfectly, and the poor imperfectly; and if this were the fact, the utility of our vaccine establishments must be more than questionable, as their practice is almost exclusively confined to the lower orders of the community. But, Sir, this fact can be satisfactorily explained, and is upon a little reflection quite obvious. It is well known the higher orders of society uniformly availed themselves either of inoculation or vaccination; and as they all attend schools, and even in their amusements are still amongst themselves, it is evident small-pox contagion cannot reach them so readily; but wherever they have been placed in the same circumstances, the same result has uniformly followed; and now these orders of society are taught from experience, their tenderest concerns have been no better protected than their neighbours. Indeed this fact, far from being employed as a bait and recommendation, ought, on the contrary, to be considered as the strongest argument against the practice. I am afraid those very individuals who flatter themselves with this exemption, and reject with disdain, and even horror, every attempt to expose the imperfection of the vaccine inoculation, will ultimately prove the only sufferers,—as they may avoid the influence of the small-pox contagion, while the power of vaccination is yet capable of mitigating the variolous influence, and may be afterwards infected with small-pox, when all the influence of the vaccine disease is exhausted, and left completely at the mercy of the epidemic.

Upon the whole, when we consider the great number of cases which have been defeated and rejected upon the score of chicken-pox, horn-pock, flea and bug bites, slight feverish attacks, imperfect, spurious, and unconstitutional vaccination, together with the steady and irresistible proof which experience now affords, there cannot exist a shadow of doubt, that it would be more consistent with truth to state, that those cases which have hitherto been supposed to resist the small-pox contagion, have either not been exposed to the influence of that disease,—have not arrived at that distance from vaccination which allows the constitution to be influenced by the small-pox contagion; may be those who have already been subjected to its influence, but have passed unobserved; or who, from peculiar circumstances, may resist the influence of small-pox at one time and suffer from it at another; or who are not susceptible of small-pox at any time.

The next reason I shall examine is, "that the small-pox subsequent to cow-pock is a much milder affection (a small proportion of cases excepted) than the slightest cases of inoculated small-pox; and that the chance of death is much less than even in inoculated small-pox." The answer to these reasons has in a great degree been anticipated from what we have already stated; but it may here be observed, that these reasons cannot be allowed the smallest weight, as all the proof which the subject is now capable of, distinctly shews, that not only has this number of cases of small-pox succeeding to vaccination, progressively increased every succeeding year, but also have become in the same ratio more severe and dangerous. Besides, it must be remembered, that as all the phenomena which attend the vaccine process, exhibit a partial and negative tendency; as the antivariolous power which it imparts to the constitution is found directly, and for a year or two after uniformly, to resist the influence of small-pox; and as experience has confirmed, that the variolous contagion exerts its power over the constitution in a few years after, in a ratio progressively increasing with the distance from vaccination, no other conclusion can possibly be drawn, with any regard to truth or fair induction, than that, in a few years more, the antivariolous influence may become *completely extinct*, and the individual left exposed to the full operation of the epidemic. Indeed, it is not to be forgot, that if a numerous, severe, and even fatal attack has already been found, not unfrequently, to have occurred at the distance of eight or ten years after vaccination,—what reason have we not to tremble when we shall come to be removed twenty or thirty! The idea is dreadful in the extreme; and, I have already remarked, no comfort can possibly be obtained from the few solitary cases brought forward by Dr Jenner, where it was stated that the constitution had been found capable of resisting the small-pox thirty or forty years after.

With regard to the argument, founded upon the small number of deaths which have hitherto taken place in those instances where small-pox have succeeded to cow pock, I must first observe, that the number of deaths are not so small as the abettors of the vaccine practice wonld have us to believe; and the number of deaths have not only been considerable, but are also rapidly increasing in the same ratio with the failures. It is no answer to be told, where death has ensued, that the vaccine inoculation had not been properly conducted. It is impossible now to pay the smallest attention to the arguments and assertions of the abettors of vaccination, as they have been under the necessity of confessing themselves wholly in error; but even allowing that such cases were the consequence of any imperfection in the vaccine phenomena, still it is equally the fault of the Jennerian practice, if these deviations and imperfections cannot possibly guarded against, or controlled by any medical man whatever. But, Sir, even allowing it really is the fact at present, and were to remain so, I apprehend the cases are widely different. It is certainly the duty of parents to do what appears to be most conducive for the safety of their child, but it must also be recollected, that the immediate ease and comfort of either the child or parent, is not the object in view. The intention is, *to put the child in such a state, as to enable him to move through every situation in life, without being afterwards exposed to danger from an attack of small-pox*;—therefore, if the parent should continue to select the vaccine inoculation, notwithstanding its evident imperfections, I should conceive he is acting very improperly with a view to the future comfort of the individual who is most interested. In the one, you adopt a decisive practice attended with a known risk; in the other you have recourse to a substitute, not only precarious and uncertain, but imparting only a temporary and doubtful security. In the small-pox inoculation, you can enter upon all the duties of social life without terror or apprehension; in the vaccine you have not only the certainty of an attack of small-pox, but even the chance of death hangs over you, and blasts all the comfort and happiness of life.

We come now, Sir, to a reason which is made use of by the determined supporters of the vaccine inoculation, which, to my apprehension, is the most singular deviation from the result of general experience to be met with in the whole history of medicine,—I mean, where they allege "that the security imparted either by the inoculated or natural small-pox, is not greater than what is obtained from the vaccine;" or, in other words, "that small-pox are as liable to occur twice in the same person as where small-pox are found to occur after cow-pock." Indeed, this reason appears to me so ridiculous, and destitute of all truth, as not to deserve the smallest attention. But besides what we have formerly noticed, which may be considered as a sufficient answer to so strange an aberration from all experience, we shall also observe, the experience of a thousand years of the natural, and nearly a century of the inoculated small-pox, have distinctly proved, that it may be adopted as a determined general fact—whoever has once passed through the small pox in a satisfactory manner, will not again be subjected to that disease. Surely, Sir, no one was ever heard of giving directions to those who had undergone the small-pox, not to go near them again, which is now done by those very advocates of the vaccine inoculation, to those who have placed their security in that practice. Whoever, Sir, heard of fathers or mothers being attacked a second time, when the disease existed among their children, so as to have the most distant resemblance to the failures from the cow-pock? Or whoever heard of individuals who were known to have had the small-pox being afraid to expose themselves to the influence of that disease? I will appeal to every practitioner of the smallest pretensions to character, if, in all the cases where small-pox have succeeded to vaccination, he ever saw any of the family, old or young, affected with the disease, but those who had undergone vaccination? I can conscientiously declare I have never either met with a single case where small-pox succeeded a second time, either after the natural disease or after inoculation; and I have seen thousands of cases of the natural disease, and upwards of twelve hundred cases of inoculation;

but I have seen several hundred cases of small-pox succeeding to vaccination in the last eight or ten years, and at this moment they are occurring here daily to an alarming extent. Besides, when vaccination was first introduced, it was announced as affording permanent protection to the same extent as was derived from small-pox, and not a single case of a second attack of small-pox was even then hinted at. It is also to be noticed, that this state of the failures in cow-pock only includes the space of sixteen or seventeen years, where the security afforded by small pox was put to the test, and had the sanction of a thousand;—and, add to all this, that the one practice arises from a source, and produces a disease, quite different from the one it is intended to combat; while the natural small-pox and the practice of inoculation in every respect resembles the disease it is stated and meant to prevent.

I shall here take the liberty of observing, with regard to Mr Hennen's cases of a second attack of small-pox, that, in the first place, they are to be justly suspected, *as they do much more than they ought to do*, and are not in the remotest degree connected with the proof, which even the advocates of vaccination have hitherto brought forward. I must also take the liberty to observe, that such is the state of the army practitioners upon this subject, as connected with the Duke of York and the Medical Board, it is but fair to conclude, their minds must labour under considerable prejudice. I have been eye witness to cases where death has taken place from the natural small-pox, while the vaccine process existed in perfection, and they were pronounced to be chicken-pox by the army surgeon. From Mr Hennen's own account, it would appear he still entertains doubts whether those were cases of small-pox which succeeded to the inoculations from his own child, and I think I may venture to assert, there cannot be a shadow of doubt about it. It is curious to observe even the anxiety of a private soldier to support the cause of vaccination; for it appears one of them readily declared he had had the small-pox before, and afterwards, considering the matter better, he told the truth, that he had not. Indeed, it is also highly probable none of the other two or three had had them; for soldiers all generally declare, when they enter the army, that they have had the small-pox, in order to avoid being vaccinated. It is to be remarked, the lower class of people have numerous pits and scars upon their body, from the diseases to which a want of cleanliness exposes them. Indeed, Mr Hennen's report is very imperfect upon this point; for neither the number, appearance, or situation is particularly noticed; and in one or two, the pits appear to be few, and they are chiefly described as being upon the trunk of the body, the very place they should not be, if the pits arose from the small-pox. Besides, Mr Hennen really seems chiefly to rest his proof of their having had the disease before upon their own authority, *—certainly, after what we have noticed, of very little consequence; and it is to be remembered also, that that disease is generally caught at a time they can be no judge of it but by report; and their testimony would certainly be entitled to more attention if it had the sanction also of their parents. Again, one or two of the number are of an age to have been vaccinated, and one is mentioned to have been inoculated eleven years ago, although there was scarcely to be found in the country a medical practitioner who would inoculate for small-pox at that period, and it is therefore more probable he had undergone vaccination.

From an attentive consideration of Mr Hennen's case No. 1. I think there are good grounds for concluding it was also a case of small-pox; for it appears he had neither been inoculated nor vaccinated; and after undergoing what Mr Hennen considered chicken-pox, he was found to resist vaccination. Indeed, it is highly probable, the child of the hospital serjeant received the infection from this case.

We must also not conclude this part of our subject without remarking, the supporters of vaccination, upon the first appearance of small-pox after cow-pox, insisted, that not only was small-pox liable to recur, but that a violent disease at first was no greater security than a mild one, and the second attack was not rendered a whit less severe, but was exposed to all the virulence of the disorder. The drift of this assertion was, to convince the public, and those of the medical profession who do not venture to think for themselves, that the very great mildness of cow-pock was no objection to its efficacy. Now, however, when it can no longer be denied that small-pox have succeeded to the most perfect form in which vaccination can be given, these gentlemen have found out, that the mildness or severity of the first attack will be followed with no increase of the second attack, but will be found to mitigate it; thereby insinuating, that the previous mild disease of vaccination will always produce a mitigated attack of small-pox. It is also to be observed, these gentlemen formerly contented themselves with producing a very small number of instances where small-pox were stated to have occurred twice in the same person; but

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* Merely because they were vaccinated, and their belief in the complete efficacy of vaccination was not to be shaken.

now, they do not hesitate to produce them in a ratio exactly in proportion to the increase of the cases of small-pox after cow-pock.

We come now, Sir, to a very curious, and, I rather think, somewhat whimsical attempt to defend the Jennerian discovery;—I allude to an opinion lately suggested by some medical gentlemen, "that variola, varicella, and modified small-pox after cow-pock, arise from one and the same contagion;" or, according to my apprehension, "are one and the same disease."

It is really, Sir, truly surprising, that this vaccine inoculation, which was once so simple, so uniform, and so satisfactory, should now become such a puzzle as nearly to overturn the most established principles in the whole history of medicine, and that every well-known fact must bend merely in compliment to those who have staked their existence upon the success of vaccination. This suggestion seems to me devoid of all support from experience and analogy; and if these gentlemen are to be allowed to change their ground in this very extraordinary manner, it will certainly be a Herculean task to compel them to surrender. I am confident, Sir, none of the established practitioners of this country, unconnected with official situations, will afford the smallest support to such extraordinary deviations from all experience; more especially, Sir, when they find you have now afforded them an opportunity to declare their sentiments without injuring their professional characters, and are at liberty to think for themselves.

I can assure you, Sir, from an experience of thirty years full practice, that I have frequently seen varicella prevailing as an epidemic, and not a case of small-pox known in the town or whole neighbourhood;—that this was the case during the last winter and spring, and not a case of small-pox existed in Musselburgh. I have also uniformly observed, that varicella regularly goes through the children of the family, with all its characters, in the same manner as variola or rubeola would do; and both before and since the introduction of vaccination, it has regularly prevailed in the families where the children had all been either previously inoculated or vaccinated. The varicella has been also equally prevalent since the introduction of vaccination, and has attacked children even a month or two after they had undergone the vaccine process. It ought also to have occurred to these gentlemen, that the disease of varicella could not have been less frequent since the introduction of vaccination, for, according to even the advocates of vaccination, cases of varicella have not only been more numerous, but also have been stated to have become fatal since the introduction of the Jennerian practice.* But I apprehend, Sir, there is not the smallest reason for torturing the proof upon this occasion, as the experience of every medical practitioner must declare, that none but the vaccinated children have been subjected to the influence of small-pox, while all the rest of the family escaped.

If more proof were still awanting, it may be observed, that if small-pox and chicken-pox proceeded from the same contagion, the chicken-pox ought also to have disappeared in the same manner which small pox did, for a few years after the introduction of vaccination, which was not the case, as has been already noticed. Again, from the phenomena which attend the varicella, they have not the most distant resemblance to small-pox; they differ most essentially in appearance, nature, duration, and sequelæ of the eruption, and most indubitably in the danger of the disease. The one requires no treatment, and the other requires the greatest care and attention,—the one is, or rather ought never to be, fatal, and I can safely say, I never saw a single case of it approach to danger, while small-pox is of the most fatal description. It is also to be observed, the experience of our best physicians has established the same facts; having been universally engaged in attending the severe and fatal disease of small-pox, while they were perfectly acquainted with the trifling disease of varicella. The term varioloid is also clearly superfluous, and only of use to produce confusion and mystery. Experience proves the disease which has occurred after cow-pock, from the influence of small-pox, either as an epidemic, or from inoculation, to be essentially small pox, as it has imparted the most distinct, and even severe disease, both in the form of inoculation and by contagion. To all this we may add, that any trials made to impart the varicella by inoculation have failed.†

There is still another mistake these gentlemen who have stated this opinion have fallen into, viz that the small-pox eruption always preserves one uniform character; but I can assure you, Sir, from the experience of some thousand cases, this idea is quite erroneous. The characteristic appearance of the eruption in all its different stages, depends entirely upon the extent of eruption and severity of fever with which it is accompanied; and there are also considerable deviations in the original appearance, as well as future progress, in point of colour, form, size, and process of maturation. Nothing is also more common, than for the small-pox to continue making their appearance for three, four, five, and even six days; and also after the fever and eruption have entirely ceased, a second crop of pustules, attended with an attack of fever, will sometimes appear as late as the sixth or seventh day; and so much am I aware of this fact, that I take every precaution to guard against any accession of fever, as I have frequently seen so great an increase of pustules produced after the disease seemed to have been determined, as to place the patient in great danger. I may also notice a well-known fact, that two or three pustules are frequently found to precede the general eruption by at least two days, and when this occurs, I have always observed rather a tedious and copious eruption to follow.

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* Which last cases, I have no doubt, were all instances of small-pox after cow-pock.

† It is somewhat surprising, and rather suspicious, that the vaccine practice should receive its principal support from medical practitioners connected with the army, whose practice and situation must certainly afford them very imperfect opportunities of judging.

But, Sir, there is an eruptive affection which I frequently meet with, and which may serve to reconcile some of these opposite opinions. It is known by the vulgar name of nirles and horn-pock. This disease I have found generally to attack children under four years of age. It is attended with slight fever and sickness for about 24 hours, which ends in an eruption of a few dozen of papulæ, which decay in two or three days, and very seldom suppurate. I have never observed this affection contagious, or prevailing as an epidemic,—was always solitary in a family,—had no connection with small-pox or varicella, as none of them existed in the neighbourhood at the time; and was quite common in my practice long before vaccination was introduced. I always attributed the complaint to an affection of the stomach or bowels, as they seemed always much disordered, and the patient was immediately relieved by an emetic and purge; and we all know, that any disorder of these organs frequently produce a cuticular eruption.

I must here advert to an observation of Dr Thomson's, contained in an article of his, published in the 56th Number of your highly respectable Journal, wherein he says, "that though Mr Brown's statement was made for the purpose of throwing discredit upon the efficacy of the cow-pock inoculation, the salutary powers of that practice in modifying small-pox seem to me to be established by his cases, (alluding to their mildness,) beyond the possibility of doubt." After what has occurred, it surely, Sir, ill became Dr Thomson, at this period of the history of vaccination, to make such an observation. It would have certainly been much more becoming, considering the situation his former assertions have placed him in, to have followed your liberal and candid example, and apologised for the mistakes he had committed on this subject, and the liberties he had used, in ascribing motives which never existed. Besides, Sir, I cannot exactly comprehend how it was possible to prove the facts I alleged, and still not to throw discredit upon the practice, when it was maintained, that no such defects attached to the practice of vaccination. Certainly the Doctor does not mean to contend, if still stronger proof should now exist inimical to the perfection of vaccination, its character is nothing impeached or deteriorated; or that it is our duty to conceal them in compliment to the abettors of that practice. But, Sir, I cannot conceive it possible for the Doctor's intelligence to overlook, that my cases were not only selected to prove the power of vaccination in modifying the influence of small-pox, but also to shew, this power became gradually weaker, and was at last likely to become extinct. If this was to be the consequence, and which daily experience is rapidly confirming, I think, Sir, you will agree with me, it is full time not only discredit were thrown upon the Jennerian discovery, but that it was entirely rejected.

I have now, Sir, nearly got through the different reasons urged by the zealous supporters of vaccination, for our persevering to place our confidence in the vaccine inoculation; but, before concluding, I must also notice the very improper defence which is attempted to be obtained by dragging forward the experience of other countries. It surely, Sir, cannot be fairly contended, that the country in which the disease has had its origin, and, by the exertions of which it has been extended over a great part of the globe, can possibly be deficient in that skill and attention necessary for tracing its effects and consequences; and that, after having introduced the disease, we must surrender up our qualifications for farther observation, to the medical practitioners of other countries, and only in future be directed by them. The doctrine is so monstrous and absurd, as to require no further comment. On the contrary, Sir, I apprehend, a heavy reponsibility rests upon the medical profession of this kingdom; and it is our peculiar duty to watch over the practice with the most anxious and disinterested concern, that we may not only guard our own, but the security of those whom we have encouraged to place their confidence in the Jennerian discovery, and not allow ourselves to be detected and exposed by those who, in this disease at least, we are entitled to take precedence of; for it is particularly to be remembered, that the vaccine practice is exactly following the same course it has done in this country, and cases of failure are rapidly cumulating in every situation; and notwithstanding the absolute power of the late tyrant of France, and the means which he employed to render it universal in his capital, Paris is at this moment suffering under a severe epidemic small-pox, and many instances of failure daily occurring. *

I must also notice, that the determined friends of vaccination, with a view to promote that practice, represented the small-pox inoculation as replete with danger and distress. The consequence of such conduct has been, that those who from experience found no security from the practice of vaccination, were prevented having recourse to inoculation, and, of course, neither used the one or other, and determined to await the attack of the natural small pox, and so exposed their families to the most distressing consequences. Even those who were still willing to employ the small-pox inoculation, could not have their wishes gratified, as it was considered contrary to the duty of a medical practitioner of any pretensions to character, to inoculate in any other form than that of vaccination. Nay, even the Court of King's Bench was, I hope inadvertently, made to contribute to the universal horror entertained against small-pox inoculation, and to support the character of vaccination. The National Vaccine Establishment, finding they were not able to get a bill carried through both Houses of Parliament, for preventing small-pox inoculation, and rendering that of cow-pock compulsatory, they availed themselves, at the suggestion of Lord Ellenborough, of an old law for the prevention of the spread of any contagious disease, and brought before the Court of King's Bench any of those who had their children inoculated for small pox, and who had carried them out of doors on the public road, and had them convicted as contravening the statute. I am convinced, however, the learned Lord had entirely overlooked, that law could not, with any propriety, be applied to such cases, until the practice of inoculation was forbidden by *law;* for surely there was no similarity between the spread of a disease, which it was wished to check and avoid entirely, and an expedient which had been practised for nearly a century, as a wholesome means for counteracting the ravages of a dreadful disease, which experience had taught the different nations of the world, they had but little or no prospect of avoiding. I am convinced his Lordship entirely overlooked, that it was perhaps both cruel and unjust to prevent those who chose to avail themselves of this wholesome expedient, to use the proper and necessary means for getting through the disease with safety, more especially, too, when the said law had never been put in force in cases of small-pox inoculation for nearly a century;—for certainly it would have been the height of cruelty and injustice, to prevent those who then placed their confidence in inoculation, obtaining the full benefit of such an important discovery, merely because others were so stupid or ignorant, as not to be sensible of its benefit.

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* It is much to be regretted that interested individuals can be so foolish as to introduce into the public prints pompous notices of the success of the Jennerian discovery, when they are well aware of the great doubts which now exist upon the subject.

It is also not to be forgot, that the abettors of the vaccine practice have produced a favourable impression of the benefits of vaccination, by stating, that an immense increase of the population of Great Britain has been the consequence of its introduction. Now, Sir, the fact is, that although the vaccine disease is perfectly safe, and affords protection against small-pox to a certain extent; still the bills of mortality in London, Glasgow, and Liverpool, or wherever they are kept, show there is no such saving produced by the reduction of the annual number of deaths, and that the mortality, upon the whole, annually, is fully greater than before the Jennerian discovery was introduced. If, therefore, such is the fact, it is possible, that while the practice of vaccination is capable of mitigating, to a certain extent, the disease of small-pox, it may increase the severity of some other diseases;* and, if this is the case, it does not signify whether our families are cut off by small-pox, or by measles, fever, &c. Indeed, Sir, I have been long of opinion, that the influence of cow-pock is of a sedative nature, from what I have observed in the measles. It has appeared to me, that the attack of measles, in those who have been vaccinated, is frequently deficient in the fever necessary to produce a speedy eruption; and I have been in the habit, for these many years past, of assisting nature to bring the disease to this crisis, and have generally succeeded to conduct the patient safely through the after stages of the disease. I may also take notice of a fact, which may serve to throw some light upon the immense increase of population, which the last census produced, viz. those who were employed to take down the numbers in a great many instances, if not in all, took down the numbers which *belonged* to a family, and not those who actually *formed* the family at the time, by which means, a vast number were taken down twice. The consequence of all this foolish and criminal conduct has been, that, for these six or eight years past, the ravages of small-pox have been nearly as great as before the Jennerian discovery was introduced.

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* I think I have observed an increased severity in scrofulous cases, and a more early occurrence of phthisis pulmonalis; and my experience corresponds with those who think the measles are rendered more severe and fatal.

I have now, Sir, brought the observations, which appear to me necessary upon the present state of the vaccine inoculation, to a conclusion; and I trust I have made out a case, which calls for the immediate attention of the medical profession. From the opinions I at present entertain, I feel highly alarmed for the public comfort and happiness, and deeply anxious for the dignity and honour of our profession. If the present proof shall be confirmed, society must be thrown into universal consternation. The parent, the husband, and the child, must be exposed to the most dreadful state of anxiety, distress, and danger;—their intercourse with the world must be timid, limited, and vexatious; and every source of comfort blasted by the most distressing apprehensions.

The length of time we have already persevered in the practice, involves the most troublesome and distressing consequences, and if we shall still go on, without a strict and impartial inquiry, we shall not only lose all claim to intelligence and integrity as a profession, but certainly expose ourselves to the charge of folly and guilt. In my opinion, Sir, the proof which attended this subject, eight years ago, was such as ought to have made us come to the same conclusions we have now done, and the profession would have saved themselves and the world much trouble. I trust and hope, Sir, we shall no longer be overawed by the opinions of constituted authorities; neither shall our having formerly acknowledged, without reserve, the perfection of the discovery; nor our granting national rewards; nor our anxiety for preserving the accuracy and consistency of the profession; nor the necessity of renouncing the violent defence which we made to the equally violent and most unreasonable efforts of the antivaccinists, be urged as an excuse for refusing to confess our errors. Fortunately, still much may be done by instituting a public inquiry into the present state of vaccination. The medical profession too, in this kingdom, will, by so doing, be in a great measure discharged from censure, and will prevent their brethren, in any other part of the world, from obtaining the pre-eminence of having detected and exposed our blindness, obstinacy, and credulity. Both Parliament and his Majesty's Government have entrusted this subject into the hands of an establishment, naturally concluding, they would anxiously watch over the public interest and safety. I am afraid, however, this subject affords but too many proofs of the alarming influence of opinion, and how readily the public interest may be sacrificed to the systematic and party sentiments of individuals. Indeed, Sir, so much am I convinced of the necessity of an immediate investigation, that, as Dr Jenner's best friend, I would advise him to come forward, and acknowledge the adverse circumstances which are now found to attend his discovery. It is evident, Dr Jenner's character could not be hurt by such a measure. Every medical man, of the smallest pretensions to candour, must allow it possesses antivariolous powers, of a remarkable nature; that the phenomena which attend the discovery, were sufficient to encourage the most flattering prospects; that Dr Jenner acted as a correct inquirer, and is fully entitled to the public remuneration he obtained.

The question, Sir, appears now, indeed, to be contracted into a very narrow compass; and, in order to come to a perfect understanding upon the subject, we have only to investigate the following queries:

1st, Can the circumstances, which experience proves, attend the vaccine inoculation, as an uncertain, feeble, and temporary antidote against small-pox, be in any way guarded against and prevented?

2d, Can the practice of vaccination be still recommended for general adoption, notwithstanding the defects which are now known to accompany it?

3d, Is it possible, with our present proof, to expect the extinction of small-pox, by persevering in the vaccine inoculation?

4th, Could the practice of vaccination be trusted to as effectual for the public safety, against the ravages of small-pox, if assisted by laws similar to those we employ in counteracting the plague?

5th, In the event of cow-pock being found inadequate for the purposes for which it was introduced, what are the best means for remedying the bad consequences which are likely to happen those who have placed their security in that practice?

It is both unnecessary and superfluous for me to enter at present into the examination of these queries. I can assure you, Sir, I am no farther an enemy to vaccination, but what proceeds from an attentive observation of facts, and which a sense of duty has compelled me to make public; and I shall think it my duty to continue the practice, until publicly and generally allowed to be improper; but I frankly declare, I think it would be sporting with the happiness, comfort, and lives of the public, were we to continue to place our security against the dreadful disease of small-pox, by employing the vaccine inoculation attended with its present defects; and, in order to put an end to all the violent disputes upon this important subject, I beg leave to suggest, for your consideration, the following plan for obtaining the necessary investigation.

I should recommend, as an indispensable preliminary, that the investigation should be taken entirely out of the hands of the members of vaccine institutions. I would advise, that the medical colleges of London, Edinburgh, and Dublin, should appoint a committee from their colleges of physicians and surgeons, of such number and character, as they know to be best fitted for so important an inquiry. That each of these committees should collect the proof which their respective kingdoms afford, and transmit such proofs, with their sentiments thereon, and the number of votes and divisions upon the different points which the subject involves, to his Majesty's government, and request the said proof to be submitted to a committee of both Houses of Parliament, and for them to take such steps, as in their wisdom they may think fit.

In order to obtain satisfactory information from the different practitioners, I would propose, they should be requested to answer the following queries; but taking care, in the first place, to inform them, they wish them to lay aside all former opinions and prejudices in favour of cow-pock; that they do not wish them to avail themselves of the excuses of perfect or imperfect; local or constitutional; genuine or spurious; chicken-pox or bug-bites; but to answer the queries, according to what has occurred to them, and henceforth, to report to the said committees every case which shall bear upon the subject.

1st, How many vaccinations, upon a rough calculation, may you have conducted?

2d, Do you not consider, that, having obtained a vesicle with an areola, succeeding to and attending it, you have imparted the disease in its best and satisfactory form; and that, if you either immediately vaccinate again, or inoculate and expose them to small-pox contagion, they will be affected with neither?

3d, Does your experience enable you to say, that although the vesicle and areola may vary in size and colour, still the antivariolous influence is communicated to the constitution?

4th, Are you aware of any ability or dexterity in a medical practitioner, whereby he can undertake to conduct the process of vaccination, without being occasionally liable to undergo sensible variations in the phenomena which that disease assumes?

5th, Have you met with cases of small-pox succeeding to vaccination, where the characteristic features of small-pox were distinct and well defined, and what number?

6th, Have you met with cases of an eruption, which appeared to resemble small-pox, and the small-pox only epidemic in the neighbourhood or family, and what number?

7th, Have you met with a trifling eruption of papulæ, which only kept out three, four, or five days, and no other eruptive disease prevailing in the neighbourhood, or in the house itself, except small pox, and what number?

8th, Did the extent and distinctness of eruption correspond with the distance from vaccination; always, however, bearing in mind, that the violence, extent, and mode of application of the epidemic, might have a considerable influence in affecting the attack of small-pox, which succeeded vaccination?

9th, At what age did you observe the attack of small-pox to succeed vaccination, and at what distance from vaccination?

10th, Where small-pox was either introduced into a family, by attacking those who had been vaccinated, or by attacking those who had neither been vaccinated or inoculated, have you seen those who had previously undergone either the natural or inoculated small-pox, again attacked at that time by the disease?

11th, If you have seen any instances of the second attack of small-pox in the same individual, you will notice particularly, whether you have seen both attacks yourself, and if not, upon what authority?

12th, Do you consider such cases of a second attack of small-pox, equally numerous with those cases where small-pox have occurred after vaccination?

13th, Do you see any necessity, either for introducing a new term for the disease which has succeeded an exposure to small-pox contagion after vaccination; or for supposing that such disease can be considered as chicken-pox; or that small-pox, chicken-pox, and small-pox after cow-pock, are from one and the same contagion?

Lastly, If there is any other information, which you may consider of importance on this subject, which answering these queries does not embrace, you will be so good as state it.

This, Sir, or some such plan, I should conceive likely to produce a result that might satisfy all parties; and, I firmly trust, you will use your endeavours to accomplish so desirable an event. After the liberality and candour you have evinced on this subject, I have no hesitation in declaring my firm conviction, that you will not only attend to my interest on this important question, but will use all your influence to obtain an impartial investigation, and will no longer allow the lives and happiness of society to be sported with. Every day produces some new and striking evidence, and calls loudly for a distinct, explicit, and decisive declaration. Every hour that passes is accumulating the anxiety, distress, and danger of individuals, and increasing, in an incalculable proportion, the public confusion and disaster that must ultimately ensue.—I am, &c.

Musselburgh, 16th November 1818.

Note by the Editors.—The weight given to our opinion and influence by Mr Brown, renders it necessary for us to say a few words in explanation. When we stated that we had become partly converts to Mr Brown's doctrines, which we had formerly strenuously combated, we had chiefly in view our change of opinion in regard to the absolute antivariolous power of vaccination. On some other points we also agree with Mr Brown ; but we still differ from him on some of the most important, especially the effects likely to result from the introduction and continuance of vaccination.

XI.

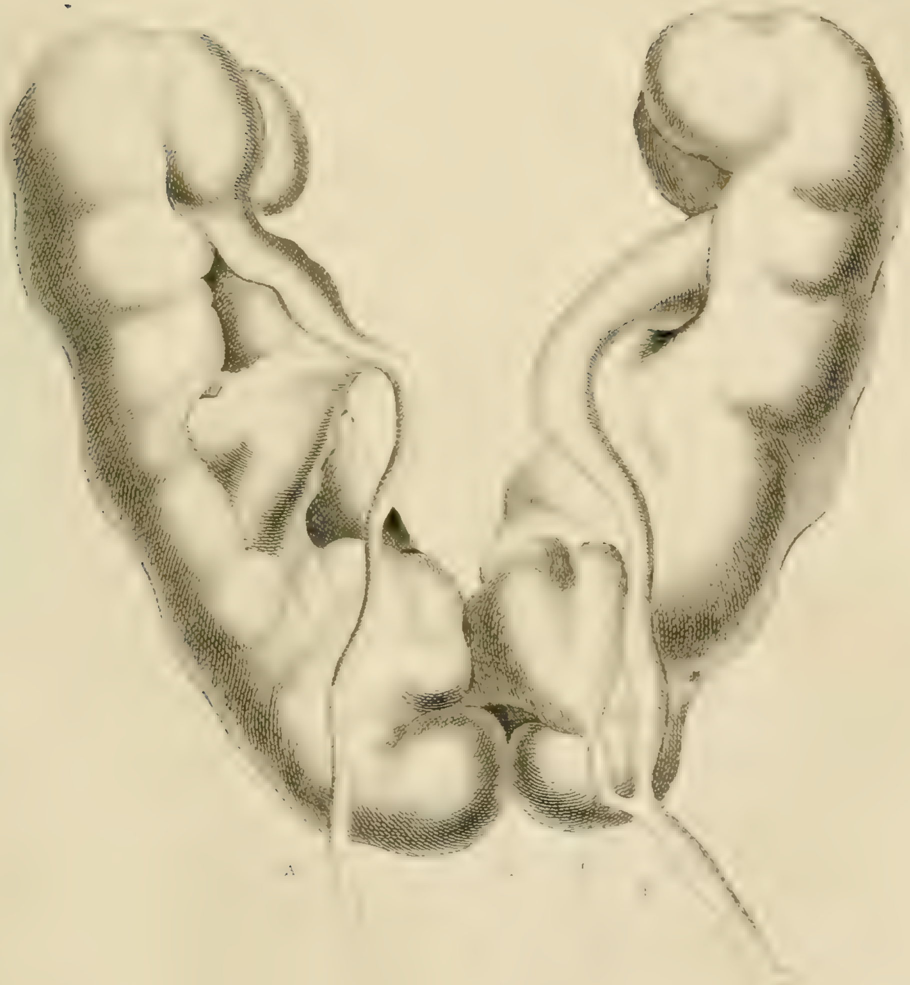
Case of Malformation of the Kidneys. By ANDREW SMITH, Army Medical Staff. *(With an Engraving.)*

JOHN BERRYMAN, aged 38, 14th regiment of foot, was admitted into hospital on the 17th August 1817, labouring under remittent fever, which proved fatal on the 9th November. On opening his body, in addition to the morbid appearances usually observed in such cases, the following remarkable formation of the kidneys was detected, which no symptom previous to his death held out the least reason to expect.

These bodies, when examined *in situ*, after all the surrounding viscera, with the exception of the urinary bladder, had been removed, showed some resemblance to the figure of the letter V, being regularly united at their lower extremities on the third and fourth lumbar vertebræ, over the aorta descendens and vena cava inferior, to the former of which they were connected by an arterial branch of about the size of a crow's quill, (in addition to the regular renal arteries), which entered their substance immediately at the line of union *D*, marked both on their anterior and posterior aspects by a sulcus or groove, the former of considerable depth, the latter very slight; but, upon the most minute examination, no corresponding vein could be detected. From the point of junction they gradually separated from each other,

Ed. Med. & Surg. Journal Vol. XV P. 90.

MALFORMATION of the KIDNEYS.



Mr. SMITH'S CASE.

and fell down on the quadrati lumborum and psoæ magnæ muscles, along the sides of the spine, so that their upper extremities were at a considerable distance from each other, and gave rise to the similarity of figure previously mentioned. They measured, from their upper extremities to the lowermost point, about seven inches; the right if any thing was the longest. Their circumference was also a little more than usual. The renal arteries and veins were quite natural, as were also the right and left ureters *A* and *B*; with these exceptions, that their coats were very delicate, and that the left was joined by an additional ureter, *C*, the elongation of a pelvis equal in size to either of the others, and formed by the union of two small branches from each kidney in the groove formerly mentioned, as marking the line of union on their anterior aspect. As few individuals, comparatively speaking, will have an opportunity of examining the preparation, exhibiting the appearances just described, which was sent through Mr Warren, Inspector of Hospitals, to the Army Medical Museum at Chatham, founded by Sir James M'Grigor, I have inclosed a drawing of these bodies reduced to half the natural size, taken immediately after their removal from the body, which will, I hope, convey to the reader a more clear idea of this variety of malformation than would result from mere verbal description.

Remarks.—The records of medicine, although they furnish us with numerous instances where nature has deviated, and with apparent impunity, from her usual course in forming these organs, yet, amongst them all, I have not been able to find one which exactly resembles that variety just described, as must be seen from the subsequent quotations I have made, the instances mentioned therein being these which have the strongest claims to similarity.

Haller* says, "Non raro est duas in uno rene pelves esse corticea natura seperatas, ut quasi duo renes in unum componantur. Imo quatuor pelves et quatuor ureteres Panoralus vidit. Duo tunc in eodem latere sunt ureteres, qui tamen plerumque confluunt Etiam extra hanc fabricam non raro fit ut ureteris duo rami longe disjuncti ad magnam a rene distantiam coeant, demum etiam seorsim in vesicam urinariam aperiantur; non in rene duplici solo sed in simplici ejus vitii ortum, vir C. mechanice explicat ex ramorum celeriori incremento, quam est trunci. Unicam fuisse ureterem rarum puto." Ruysch † gives five cases of double ureters; in three of them they united previous to entering the bladder, in the other two they perforated that viscus separately. Case 1st. —" Quamvis etiam simplices communiter sint ureteres tamen contrarium in rene sinistro duobus ureteribus instructo, in cadavere ante tres annos publice demonstravi, quorum unus ex superiore, alter ex inferiore renis parte erumpebat. Hosce ulterius denudavi reperique utrumque coire ante suam in vesicam insertionem ita ut unico canale vesica intrarent. Case 2d.—Posthac cadaver dissecui in quo observavi duos ureteres prodire ex uno rene quorum utrerque separatim vesicam intravit. Cases 3d and 4th. —Mulieris cadaver dissecantes observavimus renem dextrum duos obtinere ureteres, qui antequam ad vesicam pervenerint uniebantur, adeo ut vesicam simul perforarent. Talis renis structuram me nuperrime quoque vidisse apud Dn J. Stalpart vander Wiel, med. doct. et anatom. accuratum, memini." The fifth case is a description of a preparation of the urinary bladder, perforated by two ureters on the left side and one on the right. Besides these, similar cases are to be found in the works of Morgagni,[*] Fallopius,[†] Eustachius,[‡] Sandifort,[§] Riolanus,[||] Voigtel,[¶] &c. With respect to union of the kidneys at their lower extremities, few cases appear to be recorded. Stalpart vander Wiel[**] saw one instance in a fœtus, and two of a similar description are mentioned by Walter,[††] in both of which the ureters were natural. Dr Baillie,[‡‡] in his work on morbid anatomy, says, " The two kidneys are sometimes found to be joined together," but at what particular parts he does not specify.

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* Haller's Elementa Physolog. Human. corp. Vol. VII. page 255.

† Ruyschii Observat. Anatomicæ. No. 23 and 79, et Thesaurus Anatomic. 7. No. 32.

Such, then, is a concise account of the principal cases which most resemble that described in the commencement of this paper, from the works of the most celebrated authors both ancient and modern who have treated of that subject ; and, by comparing them together, it will be seen, that the case of John Berryman exhibited the same appearances as were done by any two of the others, and consequently that it forms a more complete malformation.

Queensberry Depot Hospital,
27th November 1818.

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[*] Morgagni, Epist. Anatom. Med.

[†] Fallopii Observat. Anatomicæ, p. 179.

[‡] Bartholom. Eustachii, Opuscula Anatom. p. 52.

[§] Sandifort, Thesaur. Observat. Anatom. Vol. I. p. 309, 347.

[||] Riolanus, Observat. Anatomicæ, p. 148.

[¶] Voigtel's Pathological Anatomy, Vol. III. p. 220.

[**] Stalpart vander Wiel, Obs. rar Med. Anat. Ch. Vol. I. p. 215.

[††] Walter's Museum Anatomicum, Art. ren. p. 97 and 265.

[‡‡] Baillie's Morbid Anatomy, p. 300, edit. 1818.

PART II.

CRITICAL ANALYSIS.

I.

Elements of Pathology and Therapeutics; being the outlines of a work intended to ascertain the nature, causes, and efficacious modes of prevention and cure of the greater number of the diseases incidental to the human frame; illustrated by numerous cases and dissections. By CALEB HILLIER PARRY, M. D. F. R. S. Member of the College of Physicians of London; Member, and formerly President, of the Royal Medical Society of Edinburgh: one of the Physicians of the General Hospital at Bath, and Physician of the Casualty Hospital, and Puerperal Charity in that city.—Vol I. General Pathology.—Octavo, pp 463. London, 1815.

THREE years have elapsed since this important and elaborate treatise was presented to the public. After having so long delayed to notice it, perhaps we ought to set out by defending ourselves from the imputation of insensibility to its high merits, an imputation to which our conduct may have exposed us. We shall, however, spare all protestations on that head, hoping sufficiently to vindicate our discernment in the sequel. We also forbear to trouble our readers with any explanation of the circumstances to which this delay has been owing: in fact, the detail would both be irrelevant and unnecessary, for as Dr Parry's is a work of all times, embracing subjects not of transient attraction, but general principles of permanent interest, it is a matter of extremely little consequence at what particular moment the analysis of it be taken up

Before entering upon that analysis we may be permitted to say, in general terms, that the book is a model of profound investigation and luminous exposition, and will, in all probability, secure to Dr Parry a place amongst those medical authors whose works are to be read for ages. It displays, indeed, a profusion of talent, and a superabundance of observation, and proves how eminently the author must have excelled in that high, though difficult requisite of a great physician—the faculty of detecting, disentangling, and classing obscure, complex, or opposing phenomena, and of eliciting elementary views from a skilful induction of isolated facts never before placed in the same light, although in themselves sufficiently familiar. Even the errors of the work (if such indeed they are) are not the errors of a writer of secondary capacity; but bear the stamp of an understanding of the first order, dazzled now and then, perhaps, and led astray by the brilliancy of its own conceptions.

The work consists of 1023 aphorisms of various lengths, rising out of, and following one another in pretty regular gradation, and marked by Roman numerals. Though we admit that an author of Dr Parry's ability is, generally speaking, the best judge of his own arrangement, having contemplated his subject in all its bearings, still we cannot but regret the choice he has happened to make, as being, we think, by no means happy: for we greatly fear that, by his neglecting the ordinary mechanical divisions of sections and subsections, his illustrations have often been needlessly incumbered.

As a necessary introduction to the chief subject of the work, he has given a very able and spirited account of the anatomy and physiology of the sanguiferous system. Contrary to Bichat, Dr Wilson Philip, and most modern physiologists, he maintains that the heart alone is sufficient to carry on the blood through the entire march of the circulation, without any aid from the muscular power of the arteries or the oscillatory power of the capillaries,—an opinion which he has still farther illustrated in a subsequent work on the arterial pulse. Indeed, he denies such powers altogether, and contends that if they existed, they would operate as much in repelling the new unda of blood, as in propelling the one already contained in the vessels. We do not wish to take up this controverted subject, yet we cannot help observing, that the ingenious author, in making the objection just stated, seems to have overlooked the known property of fluids to press equably in all directions, and the consequent necessity there is for the blood to move forward when urged by the revertibility or contractility of the larger arterial branches on the side, its reflux being prevented on the other, by the semilunar valves in the ostium of the aorta. It appears to us that the arteries have a vermicular motion,—that their circular fibres, like those of the œsophagus, contract not synchronously, but in instantaneous succession; and thus invariably carry the column of blood onward from trunks to branches. We have no doubt whatever of the inherent contractile powers both of the arteries and capillaries,—powers quite independent of the heart: indeed, the author himself, though he refuses a muscular contractility to the blood vessels, in the end allows them a vital one, which he calls tonicity; and this, in effect, amounts to the same thing.

It is a first principle with Dr Parry, that all the habits of civilized society tend to produce in mankind at large an excess of nutrition, and consequently a greater plethora of the vascular system than what is either natural to the human frame, or consistent with the healthy exercise of its functions.

He contends that, in consequence of this plethora, a disordered state of the whole, or of some part, of the sanguiferous system, is by far the most frequent of all those deviations from health incident to the animal frame. This disordered state sometime consists in the circulating fluids being excessive either in quantity or velocity, or both: at other times, though seldomer, the blood is defective in either or both of these particulars.

According to the known laws of the sanguiferous system, it is evident, that the general velocity of the circulating mass remaining the same, that portion which supplies particular parts may be retarded or quickened beyond the rate of motion in the rest of the system. Again, in point of quantity—it may be in excess or defect in one part of the system, while it is in due quantity in the rest; or it may be in excess in almost the whole body, while in some individual organ it may be defective, and *vice versâ*. In short, these two circumstances of quantity and velocity, he contends, may be so combined and modified, as to cause excessive or defective force of the circulation either in the whole body or in particular parts.

But (as might *a priori* be expected) it is more particularly in the extreme vessels of the brain, lungs, intestines, or other parts of the animal frame, that this irregularity of sanguineous distribution and impulse is apt to exert mischievous effects; and, as the capillary system is an indispensable component part of every tissue in the body, the author believes it to be the substratum in which most diseases, of whatever character, take root. For in whatever organized part disorders appear, or to whatever textures they happen to be confined, they have all one circumstance in common, namely, that they are affections of the sanguiferous system and its dependencies; as is evident from the vessels of the part invariably taking the lead in all such derangement of function or of structure.

In fine, that the various degrees and modifications of excessive or defective momentum of blood, in the arterial, capillary, and venous branches of the body, are the proximate cause of almost all diseases, is the fundamental principle of Dr Parry's pathology. This principle he expounds at great length; and then, in the remainder of the volume, proceeds to apply it to the chief diseases that assail the human frame, and to explain their phenomena and causation, by means of it.

He begins with inflammation and its consequences, that disease being one of the most conspicuous examples of increased momentum of blood. After a very scientific account of the kind and order of the symptoms, and some powerful reasoning to prove that they are, in every instance, the product of augmented impetus,—general or local,—he comes to the following conclusion:

> "If, then, in every palpable case of inflammation there is excessive momentum of blood; if we increase the inflammation precisely in proportion as we increase the momentum, and diminish the inflammation precisely as we diminish the momentum; we have, I think, just right to consider the excessive momentum as an indispensable cause of what we see of inflammation, whatever may have been the more remote causes, or whatever other invisible intermediate circumstances in the constitution, or the part, there may be, antecedent to the excessive momentum which we perceive.
>
> "Neither will this conclusion be invalidated, were it even proved, according to the opinion of Dr Wilson,* that the velocity of the blood in the vessels of an inflamed part is diminished; unless it be also proved that the velocity is diminished in a greater proportion than the quantity is increased.
>
> "I have been the more minute in my attempts to establish the quality and order of the phenomena on this important subject, because the principle will serve to illustrate various other diseases, which have hitherto been considered as very different in their natures, and therefore as leading to very different practices.
>
> "It having been thus proved, I think satisfactorily, that excessive momentum of blood exists, and is an invariable cause, in certain distinct cases, of that series of phenomena to which we give the abstract name inflammation; we have a right, conformably to the laws of sound philosophy, to conclude that it exists, and is a cause in all.

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* Dr Wilson Philip of Worcester is here meant.

"That the morbid dilatation of the vessels (in inflammation) is the mere mechanical effect of general increased impetus, I am by no means disposed to assert. On the contrary, I have endeavoured to shew above, that the fulness constituting part of the local momentum often accompanies a dilatation of vessels arising from causes apparently merely acting on their own tonicity, without any increase of the *vis a tergo* from the heart; and even that the increased action of the heart often follows, instead of preceding the excessive local dilatation. All, however, which, on this occasion, I am concerned to shew is, that increased momentum of blood in a part is essential to local inflammation; and that inflammation may arise from general increased impetus, although it sometimes occurs without it." Pp. 84—86.

We believe the term proximate cause is pretty nearly discarded from modern medical nomenclature: the following observations are both just and ingenious; affording at the same time a pretty good specimen of the author's habitual tone of reasoning:

"To the conclusion thus formed, respecting the cause of inflammation, it may be objected that this cause certainly cannot be the proximate, since it has been admitted that excessive momentum is not always or immediately followed by inflammation; whereas according to the usual definition of proximate cause, it is that without which the disease cannot exist, and which existing, the disease must exist also. To this I answer,

"First, that, according to this definition, the proximate cause is, strictly speaking, a part of the disease; which implies the absurdity, that a thing is the cause of itself.

"Secondly, the very name *causa proxima*, or that which is nearest to any thing, shews that it cannot form a part of that thing; unless we admit that a thing can be at the same time a part of another, and not a part of it.

"Thirdly, Causation, according to all our conceptions respecting it, implies not only difference and separation, but also priority. This definition of proximate cause, therefore, involves the farther absurdity of being at once prior to an effect, and coexistent with it.

"In reality, on various other occasions, we consider as the proximate cause of a disease that phenomenon or process, which, according to our perception or conclusion, is next before those which appear to us to constitute the disease; whether the disease invariably follow that phenomenon or process, or not. Thus the absorption of bile is said to be the proximate cause of jaundice, because without it there could be no jaundice; although we know that bile is every day absorbed into the circulation without producing that disease. In all cases, effects must depend on the degree or force of the cause." P. 90.

Dr Parry is of opinion that inflammation, when it arises from a constitutional cause, as in gout, erysipelas, and the like, is a natural and salutary effort of the system to remove a certain fulness of habit incompatible with the right performance of the healthy functions of that individual constitution. He relates many examples where, if the habitual paroxysm of gout did not appear, or if it was intercepted or repelled by medical treatment, diseases of a worse character appeared in lieu of it, or perhaps unexpected death took place, from the overloaded vessels suddenly giving way in the brain or in the parenchyma of some other vital organ.

There can, we presume, be but one opinion that all the terminations of inflammation, with the exception of resolution, act as evacuants of the general system. Dr Parry goes somewhat farther, and maintains, that it is solely by the evacuating effects of these various terminations that the general and local momentum of blood are removed, and the disease cured. In this way he shews that the effusion of serum, the extravasation of blood itself; the secretion of pus, or the exudation of coagulable lymph or of albumen, are all modifications of that salutary process of depletion expressly designed by nature for remedying the previous evil. But although one or more of these processes always occur, and although they relieve the part affected, it does not necessarily follow that they restore the general health. On the contrary, it often happens that the means which, according to the laws of animal life, cure the disease, kill the patient. Thus, though expectoration tinged with blood indicates a favourable termination of pneumonia, a larger effusion of the latter fluid into the parenchyma or air-cells of the lungs, often proves fatal. Thus, also, effusion of serous fluid into the cavities of the brain, always proves fatal, though by it the existing inflammatory action is terminated. Yet notwithstanding these, and the like unfavourable instances, we are satisfied the process in question is beneficial in a great majority of instances, and that the theory of the curative principle is just.

Among the effusions from inflamed vessels must be enumerated a certain cream-like matter whose chemical qualities, we believe, never have been ascertained with accuracy, though it is often thrown out in gouty and other inflammations. The author conceives that when this substance is inspissated, by absorption of its thinner part, it forms chalk-stones, pulmonary concretions, ossifications, and so forth; all of which ought to be regarded as varied deposits from inflammation, acute or subacute.

We are constrained to pass over many valuable observations in the account of the phenomena and consequences of inflammation, in order to follow the author into his general considerations on dropsy and hæmorrhage, which come next in the order of discussion.

Having before proved that effusion of serum is a frequent termination of inflammation, he goes on to investigate still more closely the relation between the hydrophic and inflammatory states; and from a variety of very illustrative facts, which our limits will not permit us to notice, he makes it appear that œdema, anasarca, and the other forms of dropsy, generally result from such a state of the sanguiferous system as plainly indicates increased impulse, though that impulse may not be sufficiently exalted to produce local inflammation. Thus the œdema following scarlatina, gout or erysipelas, is the effect of the high phlogistic diathesis reigning in the system at the time.

> "It is worthy of attention that dropsy is often evidently produced, and, when existing, aggravated, by many of those circumstances which are known to increase the momentum of the blood. Thus, ascites and anasarca often follow hard drinking, though there be no disease of the heart or liver. So also anasarca is frequently caused or increased by hot weather and hot clothing. Nay I have often seen, in anasarca of the lower extremities, the leg which was nearest the fire swell more than the other. On the contrary, such swellings are diminished by external cold." P. 146.

Dr Parry considers dropsy, whether partial or general, to be, like the terminations of inflammation, a salutary effort in the constitution to restore the balance of the circulation, and relieve such a determination of blood as is excessive, with regard to the whole, or part of the system; or excessive in relation to the accidental constitution of the person who is the subject of the malady. No doubt in this, as in the former case, the salutary effort may prove fatal either by the extent or the site of the effusion, and yet the general salutary principle be unquestionable.

He altogether denies diminished absorption as a cause of dropsy, an opinion in which we feel compelled to differ from him. We readily admit that, in by far the greater number of cases, this disease is the effect of increased exhalation, caused either directly by increased momentum in the arterial system; or indirectly by diseases of the liver, lungs, and heart, impeding the free return of the venous blood, and thereby imposing a burden or *renixus* on the extreme arteries. Yet we firmly believe that after protracted fevers, or other debilitating causes, a deficiency of absorption actually does occur, partly from torpor of the whole lymphatic system, and partly from the impaired tonicity or contractility of the cellular texture throughout the body, by which, when not impaired, the circulation in the *vasa minima* is so greatly promoted:—to say nothing of the yielding state of the capillaries themselves, under such circumstances.

However, we subscribe, in the main, to the correctness of Dr Parry's views of dropsy, and regard it as no mean triumph of modern pathology, that this disease is removed from the class Cachexiæ,—that it is no longer considered as the product of depraved solids, but as a disorder of the sanguiferous system. The researches of Dr Blackall, a few years ago, and the existence of albumen in the urine in many cases of idiopathic dropsy, have opened the minds of the profession to a belief of its general alliance to inflammation; and the labours of Dr Parry will doubtless contribute greatly to the same end.

Hæmorrhage is considered to be the effect of the same general cause as Inflammation and Dropsy: and a mass of valuable observations is furnished to prove, that, in both the active and passive kinds of hæmorrhage, there is increased impetus of blood; that is to say, that in the active hæmorrhage the momentum is positively excessive, whereas in the passive form the excess is only comparative, in relation to the resisting power of the capillaries and exhalents in any given case.

When these three diseases are spontaneous, they often act vicariously for one another, and tend equally to alleviate some constitutional errors of the general circulation. This general resemblance of action and of effect, the author regards as another proof of the near identity of cause in the three diseases. The following practical deductions from the reasonings just stated deserve the deepest attention, considering the extensive opportunities of observing such cases which Dr Parry, from his local situation, has enjoyed. Our own experience in these matters, so far as it goes, exactly coincides with his.

"So also with regard to certain hæmorrhages, many persons receive a great alleviation of various complaints, such as occur from defect of gout, by a bleeding at the nose, or a hæmorrhoidal discharge. Nay, examples have occurred, in which a patient, accustomed to vernal gout, and missing the usual fit, has had erysipelas,—the next spring hæmorrhoids, and the following spring a fever, cured by blood-letting; each with equal relief to the constitutional symptoms.

"So, also, on various occasions, not only local disease, as before mentioned, but increased action of the heart, is relieved by dropsical effusions. Thus, even in hectic fever, I have seen a pulse habitually reaching 120 or 130 in a minute, reduced in a few hours to 60, by the supervention of violent anasarca in the lower extremities.

"This point, (viz. the salutary effect of dropsical effusions,) like a great number of others in pathology, is of the highest importance, not only in speculation, but as it directs or sanctions modes of practice so active, that they must be either essentially beneficial on one hand, or highly injurious on the other.

"The coincidence of hæmorrhage with dropsy is by no means a frequent occurrence. It does, however, occasionally, under different forms, exist. I have seen a long continued and large hæmorrhage from the lungs, accompanied with hydrothorax, anasarca, and ascites, and with a pulse of 136 in a minute. All were relieved together, and the patient was restored to health, as soon as, by digitalis, the pulse was reduced to 40 in a minute." Pp. 170, 172.

With regard to sea-scurvy, as one of the varieties of passive hæmorrhagy, Dr Parry puts it as a query whether there may not, at a certain period even of that disease, be evidence of such an excessive momentum, as may ultimately produce those effects, both on the solids and fluids, which are recorded as characteristic of that disorder. On this subject we are enabled, from experience, to remark, that scurvy is produced, not merely by too long confinement to salted victuals, but by the use of aqueous, unwholesome, and unnutritious diet, of whatever kind: nay we are ready to believe that it is chiefly, if not solely, by the defect of the nutrient principle in meat that has been long salted, that the latter produces the disease. Now, it is obvious that these circumstances are very unfavourable to the assumption of increased impetus of blood, either general or partial. We look upon this disease as rather proceeding from an atony of the extreme vessels, which renders them partially incapable of resisting the usual impulse of their contents. The martyrs to the complaint are chiefly those subjects in whom we should expect such an atony to occur; namely, persons who are slothful, dirty, and leucophlegmatic; whose skins are tabid, and whose gastric functions are disordered from want of cleanliness, of fresh air, and of exercise. We are satisfied, also, that the condition of the mass of blood itself is altered in scurvy. Of this, however, we shall say more hereafter.

We have heard from some persons, on whose judgment we are disposed to place great reliance, of the efficacy of blood-letting in scurvy; but we have very great doubts as to the expediency of that practice; though we are at no loss to conceive how it would lessen, or even prevent, the subcutaneous ecchymoses.

On that portion of the work of which we have already given an account, we can safely bestow almost unlimited commendation. Besides the rich record of facts and observations with which it is stored, the views of pathology are enlarged, comprehensive, and, we have no doubt, founded in truth. The next portion, however, consisting of nearly 60 pages, in which the author treats of simple excessive determinations of blood to the cutis, to the uterus, prostate and thyroid glands, to the kidneys, liver, spleen, and muscles, and to the mucous membrane, has rather a hypothetical air about it; and the strain of illustration is far from being either precise or satisfactory. We, therefore, willingly pass it altogether, and proceed to the part next in succession, where "the nervous constitution" is treated of.

To this part of the discussion the author prefixes an account of the structure of the brain and nervous system, drawn up in that rapid and dense style for which the work, almost throughout, is so conspicuous. In the description, he, for the most part, follows the method of Dr Spurzheim, on whose anatomical investigations he passes several high encomiums. Next follows a chapter on the mental faculties, which we shall also pass over.

After some observations on the effects of the mind and the passions on the sanguiferous system, and on the various secretions, in which we find a great deal of ingenuity, but little novelty, the author lays down a position that, in the present probationary state of mankind, irritations and pains of body and mind are useful to our well-being, by keeping up, as it were, a constant call on the excitability of the brain, and hardening it, by the effect of habit, against various impressions both from without and within. Whence it follows, that those who, by sedentary or recluse occupations, are exempted from these irritations, acquire a great susceptibility of the nervous system, and a liability to inordinate motions from slight causes, owing to the excitability of the brain being permitted to accumulate.

In this way a predisposition to diseases usually called nervous is established. But the causes just mentioned are not confined in their action to the brain itself, as the ultimate organ of sensibility, but, through it, influence the heart and circulating system, which, in their turn, react upon the brain, and become a concurrent cause of the augmented nervous susceptibility. Nay, Dr Parry is inclined to believe, that the exemption from pains and irritations above referred to, acts primarily on the sanguiferous system; and that the brain suffers only secondarily, or by implication. He says,

> "For the sake of illustration, let us suppose, that, from indolence and other causes, the heart has acquired an excessive morbid irritability. In this case any impression communicated to it from the brain may excite in it inordinate action, which, determining the blood with

excessive violence to the brain, may cause it to react on various other parts, and thus produce the phenomena of nervous diseases." p. 295.

By reasoning somewhat analogous, he goes on to demonstrate that irritation in the brain from sanguineous impulse, produces excessive sensibility to external impressions, headach, vertigo, insanity, convulsions, delirium, epilepsy, hysteria, hypochondriasis, tetanus, sopor, catalepsy, &c. –all of which, he thinks, are but modifications of effect from one common cause.

> " From what we are able to observe of the effects of different gradations of irritation or impulse on medullary substance, in living animals, whether of brain, spinal marrow, or nerves, we see that a certain degree of it produces pain or uneasiness, which is often propagated onwards in the course of the medullary branches. This, I think, is the origin of the aura epileptica, which begins in the brain. A greater effect is more or less of convulsion. An increase of impulse, amounting to pressure, abridges the capacity of sensation and voluntary motion. The greatest impulse of all, wholly destroys that of both. This is precisely analogous to what happens to the parts dependent on the spinal marrow, from the various degrees of irritation or impulse which are the usual causes of paraplegia." p. 361.

He attributes to the lowest degree of increased momentum that sleeplessness, and certain other uncomfortable sensations, which must be familiar to such of our readers as are in the habit of largely consuming " the midnight oil " in protracted study :

> " There is a state, in which, without any bodily uneasiness, ideas pass quickly through the mind, and compel a degree of attention absolutely inconsistent with nocturnal sleep. This state is apt to be brought on by excessive bodily or mental exertion, by anxiety, by late hours, hot rooms, and by spectacles, which combine the two latter causes with long-continued attention, much sitting in a confined place, and a frequent succession of objects which dazzle the eyes. It is usually accompanied with increased action of the heart; the feet are often cold, and the pulse in the carotid arteries is preternaturally strong. Under these circumstances, sleep has been, on numerous occasions, induced by lying on one side, and making with the thumb a firm compression on one carotid artery." p. 298.

As a farther confirmation of his general doctrine the author instances epilepsy, which, he thinks, may be regarded as a good specimen of the whole tribe of nervous diseases, seeing it unites in itself the characteristic symptoms of almost all of them. The attacks of this malady are always brought on by causes that excite the force and rapidity of the circulation, such as, vehement exercise, exposure to the sun, excesses in food or drink, *venus nimia*, &c. &c. Its paroxysms are often caused by palpitations of the heart arising from disagreeable smells, fear, surprise, and so forth; nay, the author even doubts whether palpitation be not a prelude to the fit in most cases; although, in many, the approach of the paroxysm is so rapid that the true series of symptoms eludes or escapes observation.

The sopor consequent upon each fit, he believes to be mere collapse, or debility of the brain, produced by long continued or excessive irritation from violently impelled blood. In fact, the brain often seems to be so exhausted, that the sopor remains, while the over-impulse continues. Where exostoses or other organic diseases within the cranium are found in epileptic cases, after death, Dr Parry believes them to have operated merely as circumstances giving predisposition to the paroxysms; for as the latter are still periodical, notwithstanding the organic changes, it is to be concluded that sanguineous irritation continues to be the proximate cause.

We may observe that this view of epilepsy does not take into account, nor attempt to explain, those cases that arise *sympathetically* from dentition, worms, or other irritations in the primæ viæ, or from diseases of the spinal cord.

Insanity Dr Parry divides into moral and physical; the first originating in purely mental causes, the last in disordered circulation, or some other disorder of cerebral structure. In many instances, the former evidently superinduces the latter; were it not so, insanity from moral causes would subside with those causes, and in time cease spontaneously. In this view of the subject we perfectly agree with the author. That mania, from whatever predisposing circumstance it may proceed, is either primarily an organic affection of the brain, or ultimately becomes so, is proved by its analogy with other diseases, in which we generally observe that the disturbed function of any organ, if not already the effect, soon becomes the exciting cause, of impaired structure; and it is farther demonstrated by mental alienation being so very rarely susceptible of cure except at the outset. Yet we cannot agree with the author as to momentum being an invariable cause of this disorder, though doubtless it is a very common one. It was probably such a sweeping opinion that led to the disgraceful routine of practice that formerly obtained in some lunatic establishments, where patients were punctually bled and purged in the gross, on certain stated days, whatever might be the type of their respective hallucinations or the state of their bodily health; and were confined to the lowest diet, while a regimen of nearly an opposite nature united to judicious moral treatment, was indicated. This opinion, too, takes no account of those causes of insanity that are situated in the abdominal viscera; yet depraved conditions of the digestive organs are often known to propagate, as from a morbid centre, a host of diseased impressions that act sympathetically on the brain, and in some unknown way derange its sensorial function.

Many interesting remarks follow on hypochondriasis, febrile delirium, and spectral illusions. but our limits will not afford room for an abstract of them.

Paraplegia and sciatica are traced to increased impulse of blood on the spinal cord, and to undue vascularity of the sheath investing the sciatic nerve at its origin. The pain or numbness in both these cases is felt in the limbs, though the seat of the disease is higher up;—on the same principle that we feel a tingling pain and numbness in the little finger from a blow on the ulnar nerve at the elbow.

> "To this head, also, may probably be referred that painful disorder called Tic Douleureux; which seems to occupy the extreme ramifications either of the facial nerve, or of the second (or superior maxillary) branch of the trigeminus. All the circumstances induce me to attribute this pain to increased vascularity or determination of blood (perhaps amounting to inflammation) to the neurilema or vascular membranous envelope of those nerves. I form this judgment, first, from the strong analogy which the case itself bears to those before mentioned, in some of which dissection has demonstrated the cause: Secondly, from the extension of the disorder to the branches of more than one nerve in the same patient, which can scarcely be produced but through the medium of common blood-vessels; since there is no evidence to prove the extension of pain, by pure sympathy, to anastomosing branches of nerves derived from different trunks: Thirdly, from the disposition in this pain to be increased or diminished by those means which increase or diminish the motion of the heart: and, lastly, from the resemblance of curative effect produced on it, in common with those diseases which evidently arise from excessive sanguineous determination, by certain remedies, such as abstraction of heat, eau medicinale, and arsenic." p. 365.

Our own experience in this disorder countenances the above speculation. Dr Parry ascribes the relief often obtained by a surgical operation to the division of the arterial branch, and the consequent local evacuation of blood; and not to the division of the nervous twig.

Upon the whole, though the facts here stated about nervous diseases are numerous, curious, and edifying, we cannot allow ourselves to admit the author's opinions in their full extent. We think he ascribes too much to sanguineous impulse, and too little to the constitution of the nervous system itself. By observation, we know, that a number of causes acting directly on that constitution, and not through the medium of the circulation, are capable of instantly destroying the nervous influence, and thus producing death. Amongst such agents, the most conspicuous are sudden bursts of grief, the electric fluid, carbonic acid, the carburetted hydrogen, and perhaps others of the deleterious gases, and some animal and vegetable poisons.

Next follows some important pathological information on the relation of diseases to one another; 1st, by extension; 2d, by remote changes; and 3d, by conversion. By the first is meant, where a disorder extends itself, under a different name, to other parts at the same time. In such cases we ought to consider the disease itself, and its new modification, to be merely an extension of sanguineous momentum to different and even remote branches of the same arterial trunks Thus the author accounts for the pains in the thighs and feet, and the distressing cramps in the calves which often accompany cholera, diarrhœa, and other determinations to the bowels. In this way, also, he accounts for a brisk purgative, during a gouty diathesis, often producing acute inflammatory gout in the knees or feet; for sciatica being aggravated by cathartics; and for that heat of the head, and redness of the face and eyes, which accompany the whole train of nervous disorders, &c.

Under the 2d head are included those morbid states, which, having affected one part at one time, are apt, in the same person, to affect another part in a new form, at a subsequent and somewhat distant season. As exemplifying this, the author cites instances where the same patients, at different periods, shall have hæmorrhoids, headach, vertigo, erysipelas, or gout; and, on the other hand, where constitutional disposition to one or more of these affections shall end in epilepsy, hemiplegia, and apoplexy, or other diseases of increased excitement.

> "In one patient the succession, at somewhat remote periods, was gout, mania, and at last fatal epilepsy.
>
> "In another gout, epilepsy, and long continued mania.
>
> "In several instances I have seen fits of epilepsy wholly superseded by those of gout.
>
> "In another case the succession was epilepsy, gout, and fatal asthma.
>
> "In a gentleman, who was for many years a very intemperate liver, gout, to which he was long subject, entirely ceased after a glandular abscess in the neck, followed by a great discharge." P. 376.

On the 3d head, or the conversions of disease, we believe nothing systematic had been written; and we think medical science is greatly indebted to Dr Parry for taking up a subject so important, and yet so little understood. The facts here stated will tend to overawe that unscientific rashness by which the generality of practitioners are ever ready to check or cure, if possible, every complaint that comes in their way, without due regard to the ultimate effects on the constitution. We shall select some prominent examples of this conversion, in which the successive phenomena were only different phases (so to speak) of the same constitutional malady.

"In a gentleman, the pain of a node on the shin, supposed to be venereal, alternated with vertigo, and the sense of numbness in the head.

"A headach, of some years duration, subsiding, was followed by a cough, accompanied with incessant and wasting hectic fever. After the man had long been confined to his bed, and death was every day expected, the headach began slightly to return: and, as it became established, the cough and fever receded, and the patient regained his flesh, but continued subject to headach as before.

"In a gentleman long accustomed to violent vertigo, or pain in the head, these affections were constantly relieved on the coming on of oedematous swellings, without inflammation in the legs and feet.

"In another, an eruption of red papulæ always alternated with sickness and vertigo.

"On the other hand, various diseases of the head, as headach, vertigo, depression of spirits, mania, epilepsy, and apoplexy, in many instances, either immediately or soon, succeed the recession of inflammatory gout from the extremities.

"In a clergyman, slight gout receded, and was followed by a small discharge of blood from the rectum, which ceasing, was soon succeeded by fatal epilepsy.

"In a lady, a violent coryza was immediately removed by an epileptic fit.

"Another, on the going off of fluor albus, under which she had long suffered, was immediately seized with pain and weight in her head.

"As catarrhs recede, they are frequently succeeded by breakings out on the lips or lower part of the nose.

"The alternation of cutaneous eruptions, with different forms of dyspepsia, is common and well-known.

"I have seen hæmorrhage from the kidneys immediately succeed the desquamation of the measles.

"The disappearance of scarlatina is often followed by the succession of more or fewer of the following symptoms; bloody urine, arthritis, œdema of the extremities, ascites. To which may be added, in one instance which I have seen, convulsions, and, in another, epilepsy.

"An old man, who had lived freely, had a chronic inflammation of one leg, accompanied with œdema. Both were greatly relieved by the application of a tight bandage. In a few days, he was, for the first time, seized with violent epilepsy.

"In two cases, which occurred between twenty and thirty years ago, immersion of a gouty foot in cold water, which produced instant relief of the pain, and a proportionable abatement of the inflammation, was, in a few hours, followed by hemiplegia." P. 380, et seq.

The author conceives that these facts, (which, with many others of the same import, are drawn entirely from his own experience,) besides their importance in medical practice, reflect great light on the cause of diseases; for he contends, and we think with much justice, that the several forms of disorder above enumerated are vicarious affections, consisting of different modifications of one common action, (he says increased momentum,) directed, from unknown and spontaneous causes, to different parts.

Upon the whole, this augmented momentum, he considers to be the essential link in the chain of causation in nearly all diseases; except, of course, such as take their origin from mere mechanical causes, contagion, or vegetable miasmata. We have then the following digest of his preceding opinions:—

"First, excessive determination or momentum of blood to the skin, produces sweating, scarlatina, measles, erythema, erysipelas, and all the forms of eruptive diseases.

"Secondly, to mucous membranes,—Coryza, catarrh, hooping-cough, croup, sore-throat, peripneumonia notha, catarrhus senilis, bronchitis, asthma, aphthæ, dyspepsia, diarrhœa; and various other disorders of the villous coat of the alimentary canal; strictures in the urethra, œsophagus, colon and rectum, gleet, fluor albus, catarrhus vesicæ.

"Thirdly, to serous membranes,—Phlegmon, pleurisy, pericarditis, peritonitis of different parts, constituting enteritis, puerperal fever, &c.; inflammation of the tunica vaginalis testis. To synovial membranes, producing arthritis; together with the effects of these several states, anasarca, hydrothorax, hydropericardium, ascites, hydrocele, effusions into joints, adhesion, anchylosis, &c. &c.

"Fourthly, to various other membranes.—Of the spinal marrow or nerves,—paraplegia, sciatica, tic douleureux, &c. To the epithelion, deafness.

"Fifthly, to glandular parts,—Cynanche parotidæa, or mumps; swelling and other disorders of the thyroid gland, mammæ, testicles, prostate, and various other glandular parts; phthisis pulmonalis, atrophy.

"Sixthly, to the head,—Headach, vertigo, sleeplessness, common nervous affections, mania, delirium, convulsions, hysteria, epilepsy, catalepsy, inflammation of the pia mater, or arachnoides; together with their occasional sequelæ, hemiplegia, apoplexy, hydrocephalus, and other effusions.

"Seventhly, to other parts in various forms,—Peripneumony, enlargement of the heart, liver, spleen, kidneys, testicles, and uterus, with or without inflammation; fungus hæmatodes, ophthalmia, cataract, amaurosis.

"Eighthly, various increased natural discharges, not already specified,—Ptyalism, diabetes, lachrymatio.

"Ninthly, morbid depositions, not above arranged,—Scirrosities, indurations, ossifications, chalk-stone, biliary and renal calculi, and other hard deposits in different parts.

"Tenthly, Hæmorrhages, from serous, mucous, or other membranes, or parenchyma; as from the nose, uvula, fauces, lungs, stomach, intestines, kidneys, bladder, uterus, vasa deferentia, skin, liver, &c. To which may be added the various forms of purpura." P. 398, &c.

The disorders considered to arise from the opposite state, viz. defective determination of blood, are, shrinking of muscular parts, extenuation and flaccidity of the parietes of the heart, amenhorrhœa, constipation, syncope, and atrophy without fever.

The concluding part of the work, consisting of fifty pages, is occupied with "Exemplifications of salutary processes;" and may be considered a sort of appendix to the evidence adduced throughout the work of a tendency in the constitution to relieve itself from various irritations either by some topical outlet, or general means of reduction. In this chapter, as usual, great ingenuity is displayed, and many valuable facts are brought forward. We regret that our narrowing limits permit us to do little beyond alluding to, and recommending them.

The effect of exercise, and of various other motions voluntary or automatic, is exerted primarily and principally on the circulating system. These motions tend to drive forward the blood in the veins, and thus promote a free and equable distribution of that fluid throughout the system at large. The author judiciously adds:

> "It is easy to see how different the operation of these causes is from that of wine, heat, full meals, certain passions, and various other stimulants; for while these, for the most part, cause the heart to produce an inordinate momentum of blood in the several branches of the arterial system, and more especially of the head, while the venous system is only secondarily and imperfectly acted on, bodily exercise, on the contrary, urging forward the blood in the veins, admits of a ready evacuation of arterial blood into these channels; just as opening a vein, produces a quicker determination of blood from the neighbouring vessels to the wounded part." P. 416.

Among the instinctive efforts to restore the healthy balance of the circulation, and which, by consequence, should be esteemed salutary processes, the author includes shivering, convulsions, coughing, vomiting, shrieking, weeping, laughing, &c. by all, or at least some, of which epileptic and hysterical paroxysms are generally introduced or accompanied. But on these points we cannot help regarding his opinion as not a little visionary, and his reasoning as highly inconsequential.

Notwithstanding the length of our preceding extracts, we must yield to the temptation of inserting the following passage, as it contains a useful lesson against that "nimia cura medici,"—that pragmatical intermeddling spirit which we before spoke of. The votaries of *eau medicinale* and colchicum will find their account in laying it to heart.

> "If the representation, which has thus been given, be just, we can well understand why many local diseases cannot be removed, or even, in a certain degree, checked by local remedies, without the hazard of converting a topical into a more general malady, or of causing a constitutional effort on some other part; which part may be more essential to life, than that which the attempt was made to relieve.
>
> "The same evils may attend the administration of certain internal remedies, the tendency of which is not to cure the constitution, and so remove the necessity of the local disease, but merely to check the present salutary action of the system, and thus to cause only a temporary and delusive suspension of present suffering. Such, in the far greater number of instances, is precisely the action of the *Eau Medicinale* of Husson; the injurious and even fatal effects of which, local circumstances give me peculiar opportunities of witnessing." P. 451.

Such are the elements of Dr Parry's Pathology. We thought it best, for perspicuity's sake, to give a continuous outline of the whole at once, with as few critical interruptions as possible. Our strictures on the system at large we have reserved for this place, and we now venture to submit them to the judgment of our readers.

1. Our chief objection to the system, then, is its extreme simplicity. It may seem hyper-critical to impute that as a fault, which is generally considered as so high a recommendation; yet, whatever may be the merit of simplicity, in the abstract, we are satisfied that no system, which has this for its basis, can explain satisfactorily the origin and phenomena of diseases. Nature may be simple in her ends, but in the means employed to achieve them, she seems to be sufficiently complex. As general health does not consist in the right performance of one function only, or the due action of any solitary system, whether arterial, venous, absorbent, or nervous,—but rather in the general harmonious balance of all; so neither does idiopathic disease con-

sist in the derangement of one function or texture, but in the greater or less disturbance of the whole animal machine. Consequently there cannot be, as Dr Parry maintains, one common origin of all diseases, any more than there can be one universal remedy for them.

An eager spirit of generalization is the error of almost all powerful minds, and, with its usual result, hypothetical simplicity is productive of much detriment when applied unreservedly to a science so complex as the philosophy of life. A slight glance at the revolutions in medical doctrine, will convince us how much both Physiology and Pathology have been hitherto retarded in their progress by the excessive love of system, and the adoption of some one principle or agent to the exclusion of all others. In Physiology, that assemblage of phenomena which, by a compendious form of expression, we denominate *Life*, has been ascribed in one age to a *φυσις* or native occult virtue, in another to an Archæus, in another to elemental fire (*εμφυτον πυρ*:) in others, and more modern ones, to a moving principle,—a subtile and mobile matter,—a nervous fluid, a vital principle,—an electric or galvanic aura, and so forth. The belief in these marvellous and invisible energies, (which in reality explain nothing,) has prevented the phenomena of life from being received as ultimate facts, and its laws from being studied in their details; and thus has, perhaps, perpetuated our ignorance from century to century. For had these details been entered upon analytically, and the inductive method rigorously followed, it is to be presumed, that the accumulative labour of past ages would, by this time, have secured, to animal physiology, a higher rank among the natural sciences than it actually possesses at this day.

In pathology, also, who can forget the dogmatism of the Methodics, the obstinacy of the Galenists, the ravings of the Chemists, and the boastings of the Mathematicians,—all toiling at their favourite theories, and respectively teaching narrow and exclusive doctrines of disease? —These doctrines have had their day, and have successively mouldered away under the silent touch of time; and from what we know of the past we can scarcely venture to predict that Dr Parry's all pervading "momentum of blood" will fare better, as a *universal* cause of disease, than the *χωλη* of Hippocrates or the *Pituita* of Galen,—the fermentation of the Arabians, the viscidity and lentor of the humoralists, or the lemmas of the mathematical physicians. We are very far from maintaining that increased or diminished impulse of blood is not a most important principle in pathology: we say only, that the author has carried it too far, and has weakened a good cause by trying to prove too much

§ 2. We object to our author's disregard of the nervous system, and to his assigning it only a subordinate influence in the causation of diseases.* We cannot consent to receive a doctrine which exalts the sanguiferous system as all in all: because we are satisfied that, though the nervous and circulating organs are, as to their inherent powers, independent of each other, they are nevertheless associated in the most intimate relation, and exert the closest mutual influence. Nay, we think, it might be urged against Dr Parry, with arguments at least as plausible as his own, that the nascent movements of all diseases depend upon the sentient system; and that, even in inflammation, (the least questionable example of sanguineous momentum,)—the primitive excitation, or morbid impression, is always exerted upon the nerves of the part, and that determination of blood follows only consecutively, according to the well known law, " Ubi irritatio, ibi fluxus," &c.

To be sure the author, either with the view of modifying his doctrine, or of obviating objections, admits that there may be invisible intermediate circumstances in the constitution, or in the part, subsequent to the application of the remote causes, and antecedent to the excessive momentum of blood which we perceive. But notwithstanding all such circumstances, he still chooses to consider the increased momentum alone as the essential cause. Now it appears to us, that, according to this reasoning, morbid heat might be assumed as the essential cause of fever, or yellowness of the skin as the cause of jaundice, seeing that, in these diseases, heat and yellowness always precede the lesser phenomena; and unless they appear, (whatever may be the other invisible intermediate circumstances,) no fever or jaundice can truly be said to exist.

But we wave these matters as entirely speculative, and pass on to an opinion of the author, (advanced in aphorism 97,) that the function of secretion is an attribute of the circulation, accomplished by an innate power in the vessels of electing or rejecting certain elements of their contained fluid, red or colourless. This opinion is surely untenable; for it is proved by the experiments of all modern physiologists, and particularly by those of Dr Wilson Philip, that secretion, more perhaps than any other organic process, is under the direct influence of the nerves.

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* Our author's language is more than sufficiently strong: he " utterly, and forever, disclaims all reliance on the neurological systems of pathology hitherto extant. He considers them as founded on principles, which are either visionary or inapplicable, and which lead to practices, tending equally to debase the moral character of mankind, to produce, or perpetuate, disease, and to discredit the medical profession." Preface, p. 5.

3. We find throughout Dr Parry's work a total disregard and neglect of the lymphatic, glandular, and absorbent system. There can be little doubt that, in many diseases, both acute and chronic,—but particularly after measles, scarlatina, &c. there exists a general torpor of that system, which modifies greatly all the other symptoms.

4. Admitting, for argument's sake, the author's general doctrine to be true, we deem him somewhat blameable for considering merely the quantity and velocity of the blood, without attending to alterations in the quality of that fluid. This, to be sure, is a prevailing oversight with modern authors, so that Dr Parry might here plead the authority of prescription. We are disposed to grant, without the least hesitation, that the humoral pathology of former times, like all other exclusive principles, was erroneous from being carried too far. The consequence has been, that it has now fallen into utter, but perhaps unmerited, neglect. We are satisfied it had its points of merit, inasmuch as it directed to researches into the morbid conditions of the fluids, and especially of the blood. To despise such researches is certainly unwise; for this most important fluid undergoes very material changes of constitution in scorbutus and some other diseases,—changes without which the symptoms cannot be accounted for. This subject, however, is too extensive to be treated of in this place.

5. Our fifth and last observation against the system is, that the author now and then reasons from slight or feeble analogies. Indeed, much as we admire his argumentation generally, we are constrained to say that it is sometimes vague and unsatisfactory,—for instance, where he attributes to modifications of increased momentum complaints so different as spasmodic asthma, dyspepsia, diabetes, and fungus hæmatodes, diseases not only of dissimilar symptoms, but of dissimilar tissues.

It is one of Dr Parry's fundamental positions, that most of those movements, constituting what is called disease, and which, for the time, produce disorder of the different functions, whether of body or mind, are, in reality, processes, the general tendency of which is to restore, and to prolong life. But of the general truth of this proposition, though doubtless it is correct in some cases, we entertain very great doubt, or rather actual disbelief. We are in the habit of thinking that the final cause of a great majority of the diseases incident to humanity, is to be sought for in the direct appointment of Providence; and that those diseases, like other physical evils, are intended to fulfil certain inscrutable purposes, both moral and natural: of the latter, perhaps, the only obvious one is, the removal, from this stage of existence, of the successive generations of mankind, in order to make way for others; and thus give effect to that law of *change* which rules the whole animated kingdoms of this earth.

Besides, the elements by which we are surrounded often excite disorders both painful and dangerous; which, so far as we can judge by the event, are by no means productive of any indirect or ulterior advantage to the general constitution, but often, indeed, the very reverse.

Moreover, this doctrine of the salutary tendency of diseases is by no means new. It may be found laid down at great length in the writings of Stahl, who ascribed to the sovereign *anima* every phenomenon of health and disease, and assumed that every natural distemper was set in motion by that presiding principle, with a view to some benevolent purpose or change in the human system. Not only in this matter, but in the general doctrines about plethora, we think a considerable resemblance may be traced betwixt the pathological principles of Dr Parry, and the now almost forgotten speculations of the celebrated advocate of Phlogiston.

After having pointed out with such freedom what we consider the defects of this system, it behoves us to be no less explicit in pointing out its prevailing merits.

In the first place, then, we may notice the width and comprehensiveness of its principles as giving it a just claim to preeminence. A system of satisfactory pathology must be founded on a view of the whole human economy, and not on the morbid appearances or deranged functions of single parts. So far Dr Parry's answers to this description; and though he has not succeeded—(who, in the present imperfect state of our knowledge, can hope to succeed?) still he deserves the greatest praise for laying his foundation so deep, and for tracing to the pale of the circulation a number of perplexing and untractable disorders, hitherto not suspected to arise from that source. He has illustrated (at page 101, *et seq.*) the safeguards against inflammation which nature has instituted, in a manner at once original and beautiful.

In the second place, if candour required us to say that the work breathes too much the spirit of system, we are equally bound to acknowledge, that it breathes also the spirit of philosophy,—combined, too, with what is not less admirable in a physician,—a talent of acute and original observation. There are but few volumes that can display such a mass of compressed thought, and valuable practical knowledge. This, however, we have, in substance, said already.

The book defies all efforts at condensation, being already condensed to the utmost by the masterly hand of the author. The account we have given of it is but a very faint one;—nay, we fear, we must have done it great injustice, from our being obliged, for the most part, to quote only the conclusions, with little or none of the inductive reasoning by which they are premised. But we trust our readers will remedy the defect by studying the original, instead of contenting themselves with our brief analytic sketch.

The appearance of a system—particularly *such* a system—of pathology, is not an every-day occurrence: and this must plead our excuse,—if indeed any excuse were necessary,—for assigning to Dr Parry's labours such an unusual number of our pages.

It is impossible not to lament, that the great undertaking thus energetically begun, can, in all probability, never be completed by its distinguished author. That stroke of disease, which pays as little reverence to exalted intellect as to elevated rank, has unhappily paralysed the strength of his powerful mind, and prematurely quenched that fine glow of scientific enthusiasm—that resplendent genius which illumined whatever it approached. Who is there that does not sigh over the fairest attributes of man in eclipse—the touching spectacle of intellectual greatness in ruins?

II.

The Influence of Civic Life, Sedentary Habits, and Intellectual Refinement, on human Health, and human Happiness. By JAMES JOHNSON, Esq. Surgeon to his Royal Highness the Duke of Clarence; Author of "The Influence of Tropical Climates on European Constitutions," of "A Practical Treatise on Derangements of the Liver, Digestive Organs, and Nervous System;" and Editor of "The Medico-Chirurgical Quarterly Journal." 8vo. Pp. 93. London, 1818.

THE last ten years have been distinguished in a very marked degree by the improvement of our art both in matters of pathology and practice. During that time the profession at large seem to have been actuated by a desire, more simultaneous than at any former period, to overlook reigning symptoms, and to investigate particular diseases after the inductive method of the more perfect natural sciences. Among those who have honourably distinguished themselves by a talent for independent observation and successful induction, it would be injustice not to mention Mr Johnson. He is entitled to the praise of having written a work on the Influence of Tropical Climates, which excels,—and we had almost said supersedes, every other book on the subject. In that work he has struck out some new and ingenious views, both of the physiology and pathology of the liver; and, moreover, (if we may trust to the opinion of those who have practised extensively in hot climates, and who may be presumed to be the best judges of the fact,) he has introduced conspicuous improvement into the field of tropical practice. His more recent treatise, on biliary and visceral derangements from atmospheric influence, also, is calculated, from the simplicity of its illustrations, and the clearness of its arrangement, as well as the interest of its matter, to diffuse a spirit of intelligence and inquiry among the humbler classes of the profession.

Upon the whole, Mr Johnson may lawfully claim the distinction of being a useful and a successful medical author, seeing what he has published from time to time, has acquired a large share of public estimation. In fact, he may be said to have "bought golden opinions of late;"—and he seems determined, withal, to "wear them in their newest gloss;"—for we find him once more at the bar of public opinion, although but a few months have elapsed * since he was there before.

In the present essay he has tried, like Dr Beddoes, to bring home the principles of medical science to the ordinary business and habits of mankind; and the tract may be characterized as an estimate of the usual tendency of fashionable or sedentary habits in reference to the laws of animal life. He traces, in a minute and forcible manner, the effects, direct and consecutive, of "civic life, sedentary habits, and intellectual refinement," on the digestive apparatus, on the heart and circulating vessels, on the lungs and glandular system, on the animal functions, and on the brain and nerves.

The interest of the discussion may be presumed from the scope which the treatise embraces; and though it is composed in a semi-popular strain to adapt it for general perusal, there

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* See the number of this Journal for April 1818, p. 254.

are many professional hints thrown out that might afford the wisest of us subject-matter for reflection. In selecting a specimen of the work, we might almost quote at random. We shall take the following illustration of the consequences of luxurious indulgence in eating:—

"The ploughman, exposed at all seasons to the inclemencies of the skies, and strenuously exercising his voluntary muscles, might gormandize with safety on Alderman's fare. But not so the citizen, however well trained in the school of Epicurus. His sedentary life and a host of moral and physical circumstances around him, render it a matter of impossibility that *repletion* shall not succeed even an apparently temperate regimen; and, in reality, this repletion, and the irregular states of plethora which thence result, characterize nine-tenths of the diseases of civilized life, though they assume the garb of debility, and too often lead to the most erroneous and unsuccessful methods of treatment. Every one, after a full meal, especially of animal food, with all the et ceteras of a civic table, must have felt how incapacitated he was for either mental or corporeal exertion. It is a law, indeed, in the economy of the living machine, that where any one of the three systems above mentioned is over-exerted or over-excited, one or both of the other two systems must fall into a state of irregular or deficient action. The heavy meal of animal and other food exemplifies this law. When the digestive organs and circulating vessels are strongly engaged, the muscular and intellectual are indisposed towards the full exercise of their functions, the greater portion of vital energy being then apparently concentrated in the *organic* system, the principal theatre of operations for the time. On the other hand, let the animal system, or voluntary muscles, be thrown into violent or unusual action, the digestive process is diminished, or even suspended, and the mind is incapable of dwelling intently on any train of thought. This law of irregular or unequal excitement of the system, hitherto so much overlooked, unfolds the most important views, both in health and disease," &c. P. 9.

From the above extract our readers will perceive, that this little treatise is written with that facility and liveliness which form the prevailing characters of Mr Johnson's style. It abounds also in familiar, but forcible illustrations; take, for instance, the following:—

"The *over action* of the principal passions on the brain and nerves, closely resembles the over-action of food and drink on the stomach and other digestive organs, in many minute particulars, and especially by attracting an *undue proportion of blood* to the over-excited parts. The whole of the phenomena attending the Proteian host of *nervous disorders*, and all the most successful methods of treatment, attest that their immediate seat, or source, is an unequal distribution of the blood, and of the sensibility. The brain and nerves becoming more irritable, from over-excitement by the passions, their vessels swell with blood, and this local turgidity causes a constant pressure on, and keeps up a perpetual irritation in, the whole nervous system. This is a doctrine which, though deduced from actual observation, is far wide of the popular belief, and but little diffused in the medical world itself. It is of such importance, however, and opens out so much better a practice than is generally pursued, that I shall go somewhat into detail, in order to illustrate it." P. 83.

This, according to our ideas, is sound pathology; indeed, it is but bare justice to say, that the pathological principles developed throughout this little volume are highly creditable to the author. It would be difficult to point out another work of the same size, in which so many important points are discussed and explained.

At parting, we shall make free to tell Mr Johnson, that we by no means admire the taste of his so often introducing poetical quotations (some of them, too, sufficiently trite) into the body of his compositions. In the light ephemeral pages of a novel or a newspaper they are, generally, not out of place; but to the spirit of grave or professional discussion, we maintain, they are totally unsuitable. We believe the "Musæ Severiores," who preside over the details of science, are not *on terms* with the nymphs of the same name, that flaunt about the gay neighbourhood of Parnassus, idly stringing their beads, bouquets, and poesy; it must, therefore, be no slight violation of etiquette to bring them together in a manner so unusual. Though Apollo is said to patronise both poetry and physic, we apprehend he rules them as two distinct kingdoms, and wills each to subsist in distinct and separate supremacy.

Mr Johnson must not consider this unceremonious admonition as arising from any thing like the "iniquo animo;" we have a sincere respect for his talents and industry; and we only condemn the practice in question, because we think it detracts somewhat from the dignity and the value of his writings.

III.

Philosophical Transactions. 1818. P. 172. *The Croonian Lecture. On the Changes the Blood undergoes in the act of Coagulation, and Additions to the Croonian Lecture.* By Sir EVERARD HOME, Bart. V.P.R.S.

IT being our intention, in future, to attend in a particular manner to the papers relating to medicine, which annually appear in the valuable collection above announced, a more auspicious subject than the Croonian lecture for 1818 could scarcely have presented itself, nor, indeed, would it be easy to find one, since its commencement, more directly consonant to the views of its excellent founder.

The blood is a fluid which, from its manifest importance in the animal system, has, in all ages, attracted the attention of inquirers. It is only of late, however, that our investigations have made any considerable progress; and we are happy to be able to add, that the papers, whose titles we have just given, bid fair to add something of real consequence and utility to the many curious but insulated facts, which modern ingenuity and perseverance have established respecting this singular fluid.

The author commences with an investigation, founded on microscopical observations of the red globules of the blood. These he finds to measure $\frac{1}{6000}$ of an inch, according to an ingenious and apparently accurate expedient to procure this estimate, by a gentleman well known for his mathematical talents, Captain Kater.* Another estimate is previously given, as obtained from the micrometer, by Mr Bauer, who, it must be observed, has been Sir Everard's sheet-anchor in the whole of these experiments. This previous estimate gives $\frac{1}{1700}$ of an inch as the diameter of a globule while surrounded with its colouring matter, and $\frac{1}{2000}$, that is, a fifth smaller, when the colouring matter is removed. The reason of our attention to this minute particular will presently appear, when we shall have to show that those particles cannot, as Sir Everard Home supposes, go to form the fibrous textures of the body, by arranging themselves in rows; in short, that these fibres are not the one-eighth part of the diameter of one of the red globules; and, consequently, cannot be formed of corpuscles which are so much larger than themselves.

When the globules in the human serum are deprived of their colouring matter, they are seen floating, and attracting each other; often arranging themselves in right lines; and, from this circumstance it is *conjectured*, that the muscular fibre may, perhaps, be formed of a number of these globules arranged lengthwise. Accordingly, after hunting long and ineffectually for a muscular fibre that should exhibit the corded appearance which such a formation might be expected to present, the learned Baronet and his observer (whose talents he describes as being "almost beyond what is natural") were so fortunate as to discover the object of their search in the thigh of a roasted chicken. They afterwards, however, found them, by means of a little management, in the fibres of the human stomach,—in the salmon —in the thigh of a rabbit and sheep. The process consists in boiling or roasting the part, and macerating it for a week in water, which must be every day changed ; and, finally, exposing it to the field of the microscope. They then distinctly appear to be composed of a chain of globules, which are oval, or rather oblong in form. When they are broken down in water, on examination, the latter is found full of detached globules *of the same size* as those observed in the blood. The same globular structure, but more minute, was seen by Mr Bauer in the brain ; and a similar genesis is ascribed by Sir Everard to all the tissues of the animal body.

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* Considering the modern improvements in optical measurement, perhaps this should be reckoned the most accurate measure of a red globule yet given. Haller makes it $\frac{1}{5000}$, Wollaston $\frac{1}{4900}$, Dr Young $\frac{1}{4416}$.

Our readers, by this time, will have called to mind the vesicular structure, ascribed to the muscular fibre by Borelli and many others, amongst whom, for a time, was the founder of the Croonian lecture himself. The same optical appearances, or deceptions, as some suspect, may have given rise to that opinion, as to the one at present advanced by Sir Everard Home. We are not, however, quite so positive as the learned Baronet, when he affirms, that "the accuracy of the appearances may be depended on ;" for in microscopical observations, whether from the almost unavoidable aberration of the rays of light, or the scarcely less inevitable prejudices of the observer, no branch of the sciences, depending on the evidence of the senses, affords such various and contradictory results. We need but remind our readers of the different forms ascribed to the globular bodies in question. *

But an example much more to the point is still in the recollections of the hoary-headed critics that fill the upper benches of our fraternity ; we mean the mistake of the late Dr Monro of this place, who, from optical appearances, not very dissimilar to those above, not only persuaded himself, but many of the most learned men then, and now living, that the nerves, and, in fine, all the structures of the body, (and, indeed, of NATURE,) are composed of *fibres, inclosing spiral tubes, which run zigzag from*

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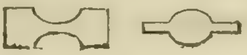
* We are indebted to his Imperial Highness the Archduke Maximilian of Austria for an account of the most recent observation of the figure of the globule of the blood by AMICI, Professor of Mathematics at Modena, who, by means of a fine reflecting microscope of his own construction, which appears to be one of the most powerful and distinct instruments ever made, has found them to be of two kinds, somewhat as in the annexed figures.



one side to the other, not unlike the winding of the seminal ducts of the testicle. In 1779 the Doctor went so far as to read a paper on the subject before the Philosophical Society; but in a short time, after puzzling all the learned of his acquaintance, and *the first practical, as well speculative, optician in Europe*, he discovered, " that the appearance of these convoluted fibres is an optical deception, arising partly from the unavoidable aberration of light in the microscope, and partly from the irregular dispersion of it from bodies whose inequalities are neither incomparably greater nor incomparably smaller than the distance at which light is acted on by bodies." * One of the elderly gentlemen above quoted, has been comparing Dr Monro's Representation with the figures now given in the Philosophical Transactions, and discovers a wonderful similarity: but as this is by no means a favourite part of the paper with us, we proceed, merely remarking, that the measure of the muscular globules above described, *does not at all* agree with the observations of Prochaska, † who says expressly, that he had found muscular fibres seven or eight times smaller than the greatest diameter of a red globule, that is, not much exceeding the $\frac{1}{40,000}$ of an inch; and that the article quoted from the Supplement to the Encyclopædia Britannica (written by the late excellent Dr Gordon) is so far from " confirming, in a satisfactory manner," Mr Bauer's observations on the size of the cerebral globules, that it expressly and flatly contradicts them. For it informs us, that Prochaska found the cerebral globules eight times smaller (just like the muscular fibres) than the blood globules; and not one-fifth smaller only, as Mr Bauer did, when he found them of the same dimensions as a blood globule deprived of its serum, and, of course, quite ready, and of the requisite diameter, to be strung endwise into a cerebral fibre.

It is on the coagulation of the blood that our author has succeeded, and the manner in which the first part of the investigation is conducted is truly refreshing, as it reminds us of the satisfactory precision and logical accuracy with which the first great men who introduced the method of induction into physical science, proceeded onwards to discovery; at a pace, slow, indeed, but never retrograde.

Mr Bauer, it seems, has long studied the germination and vegetation of plants with much attention; and on observing the growth of a tubular hair in the root of a wheat plant, which is very rapid in its growth, he found the process to take place in the following manner: On the radicle to be elongated, small pustules of a slimy substance arise under the epidermis on the extremity, and, in a few seconds, a small bubble of GAS bursts from the root into the slimy matter, which it extends in a moment to the length the hair is to acquire; which done, the slimy matter immediately coagulates, and forms a canal or tube. If the root is to be jointed, a new bubble of gas is made to blow this out to the requisite length of the *internodium*, and so on. We may easily conceive all vegetable tubes to be formed in this way.

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* Nervous System, p. 171.

† Op. Min. Part I. 160, et infra.

Transferring, by a natural process of the mind, these circumstances to the animal kingdom, Sir Everard Home first inquired whether any gas existed in the human fluids which could act in a similar manner during the formation of *animal* tubes. This question was of easy solution; for it had been known from the most early times, that air exists in the blood, and had been the stronghold of the Eristrateians, who believed that the arterial system was literally full of air, which sometimes passed over into the venous side. It had been as strenuously denied by their antagonists, and the too eager supporters of Harvey (among these Cruickshank) in the teeth of common sense, and the evidence of their own eyes.* The fact was known to Francisco Redi, to Caldesi, and described by them; but more particularly by the great Haller, who, to many other curious particulars, adds, that it is more abundant in the cerebral vessels; a fact which was long after confirmed by the dissection of the body of the celebrated De Saussure.† Our countryman Dr Hales made some rude attempts to measure the quantity of this circulating gas; but it is in the experimental essays (1767) of the celebrated Dr M'Bride (210-212) that we find the thing demonstrated and elucidated by two simple and beautiful experiments, which, besides ascertaining the presence of gas, demonstrate that it is CARBONIC ACID; and that it properly resides in the crassamentum, and is easily separated from it. Finally, Vogel‡ has shewn, that when blood is placed in the vacuum of an air pump, a considerable quantity of carbonic acid gas separates from it. But Sir Everard Home, perhaps influenced by the experiments of Cruickshank,§ again submitted the matter to experiment, and, by means of his accurate friend Mr Brande, ascertained that, under the receiver of an air pump, blood yet warm from the vein gave out carbonic acid gas, TWO CUBIC INCHES for every OUNCE of blood; that during exhaustion, there was a considerable escape of gas from the blood, so that it had the appearance of effervescing, and soon depressed the quicksilver in the gage of the pump.

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* Haller, El. Phys. I. Lib. III.

† Vid. Vol. II. of this Journal, p. 239.

‡ Ann. de Chimie, XCIII. 71.

§ Absorb. System, p. 107.

He next ascertained that the carbonic acid is chiefly evolved during coagulation, a fact which corresponds very well with M'Bride's XXIII. Experiment, shewing that it resides entirely in the coagulum.

It now remained to examine what share this gas has in forming the vessels of the coagulum. A drop of blood, fresh drawn from the arm, was subjected to the microscope.

> "In about five minutes something was seen to be disengaged in different parts of the coagulum; beginning to shew itself where the greatest number of red globules were collected, and from thence passing in every direction with considerable rapidity through the serum, but not at all interfering with the globules themselves, which had all discharged their colouring matter. Wherever this extricated matter was carried, a net-work immediately formed, anastomosing with itself on every side, through every part of the coagulum. When the parts became dry, the appearance of a net-work remained unaltered. In some instances bubbles were seen to burst through the upper surface of the coagulum; this, however, did not prevent the ramifications, that have been described, from taking place."

This net-work does not appear in serum, and in a mixture of serum and globules, is only found amongst the red globules. But granting the formation of these tubes in a clot from the evolution of carbonic acid, is there any means of identifying them with the vessels which constitute its organization? The following experiments would seem to show, with tolerable certainty, that there are; and, even if there should remain any doubt on this point, the artifice employed in its investigation is so very novel and refined, that it cannot but admit of extensive application in the further prosecution of the subject, and, indeed, in all the nicer researches of the practical anatomist.

I. From blood allowed to stand forty-eight hours, the serum was strained off, and about one-sixth of the remaining coagulum pared away. The cavity thus made was filled with common red size injection, and the whole placed under the receiver of an air pump; which, by exhausting the clot of its carbonic acid, readily permitted the size to take its place in the tubes it had formed. During this process the size was in a perpetual state of effervescence. The blood was now exposed to the heat of boiling water, with the intention of increasing its firmness, and at the same time, of melting off that part of the injection that had not passed into the clot. When sufficiently hardened it was found most beautifully injected.

II. The mesenteric artery of a rabbit was wounded, the animal allowed to live forty-eight hours. After its death the arteries of the abdomen were injected with the common minute injection, coloured by vermillion. This cavity, on being opened, was found in a perfectly natural state, without adhesions. One very small coagulum was found lying upon the peritoneum, adhering to it in parts, but not by the whole surface of contact. This was evidently injected. When subjected to Mr Bauer's powerful microscope, (400 power,) he found that the small arteries of the peritoneum entered the TUBES OR CHANNELS OF THE COAGULUM, which are much larger than the small vessels with which the communication is formed, and are easily distinguished by the peculiarity of their form. Of this any one may satisfy himself, by inspecting the beautiful engravings that accompany these articles in the Philosophical Transactions.

This simple experiment decidedly established the connection between the living circulating vessels, and those hitherto inert tubes which owe their formation to the escape of fixed air. Further experiment confirmed it. Thus an injection that Sir Everard Home had made in the year 1788, of a pedicle of coagulable lymph, adhering to the surface of an intestine, was now examined, and found to exhibit precisely the same appearances as those just described from the rabbit.

There is a preparation in the Hunterian Collection which exhibits a coagulum that had been produced by wounding an artery in the operation for hydrocele. It had been minutely injected by Mr Hunter himself; and was the most satisfactory proof of coagula having vessels that eminent man ever met with. The accident had taken place four weeks before the extirpation of the testicle, and both its vessels and air-formed channels had arrived at a period of their formation that can leave no doubt of their ultimately becoming identical. A slice of this preparation, examined by Mr Bauer, was seen still to exhibit the channels formed by the extrication of fixed air; but they were found to have now acquired a distinct COAT, *that admitted of being separated from the surrounding parts*, though still remaining much larger than the arteries with which they communicated, and by which they had been supplied with blood, and latterly with injection.

The satisfaction with which we have contemplated these experiments, has led us far beyond the bounds we usually allot to a review; but we shall now close it by subjoining an application of them by the author himself, which, to many of our readers, will not appear the least important; and terminating with a few speculations into which we have been beguiled by the very fascinating glimpse of "fairy land" that has just been presented to our eyes.

> "As the globules of pus," says Sir Everard, "are similar to those of the blood, I made experiments upon the fluid in which they are suspended, and found INSPISSATION produce the same effect on it as coagulation does on the other; that a similar net-work is formed, and apparently by the same means; since, if pus is deprived of its carbonic acid gas (of which it contains a large quantity) by exhaustion in the air-pump, no such net-work takes place.
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> "This is a fact of considerable importance in practical surgery; for since we know that inspissated pus can become vascular similar to coagulated blood, we have arrived at the principle on which granulations are formed, and from whence they derive the power of *contraction* which is found to be inherent in them. We also can account for the great advantage of compression upon the surfaces of sores; since, by that means, all the superfluous pus is removed, leaving only enough for *inspissation*, in which state the carbonic acid gas is extricated, forming channels so as to admit of its afterwards becoming vascular, and then taking on the form of healthy granulations." p. 194.

Whilst reading the papers which have become the subject of our remarks, we could not forbear asking ourselves, what is the source of all this carbonic acid, evolved so plentifully from blood during coagulation, and co-existent with it in the living vessels? Is it formed by a reciprocal chemical action of the elements of the vital fluid, within its proper channel, or is it aided by the combined operation of external agents? *Where*, in fine, and *when* is it formed? These are questions to which no prudent man, in the present state of our knowledge, would attempt to reply; and if we have thrown together some curious analogies in the following paragraphs, it is rather for the purpose of awakening the interest of physiologists on a subject which all seem to have given up as hopeless, than from any conviction in ourselves, or wish to procure proselytes among others.

Late experiments on *respiration* have shown, that, during the performance of that function, a quantity of oxygen disappears, nearly commensurate to the quantity of carbonic acid expired. We say nearly, for the most trust-worthy experimentalists have always found that *more* oxygen disappeared than is necessary to form the 3.45 parts of carbonic acid given out. No one, however, has succeeded in discovering the *source* of the carbon, or in explaining the famous Priestleian experiment, of venous blood when exposed to oxygen becoming arterial, generally with a corresponding generation of carbonic acid. It has never been at all established, that carbonic acid is formed *in the cells of the lungs*, nor have any direct experiments to that effect ever been brought forward; men of science having rather acquiesced in this explanation, from the difficulty of discovering a better, than from rational conviction. Indeed, it has always appeared to us as equally probable, notwithstanding the old-fashioned sound of the doctrine, and the opposition it met with when advanced by La Grange,* that the oxygen is really and unequivocally absorbed by the blood passing through the lungs, but that somehow or other, (probably by exhalation,) a portion of carbonic acid equivalent to this was given off at the same time from their surface. Several circumstances contributed to favour this suspicion, and principally the difficulty of accounting for so rapid a conjunction of carbon and oxygen taking place in the pulmonary cells, and that, as appears, without the evolution of sensible heat. The electrical experiments of Morrichini also, (Mem. del Acad. de Turin, VI.) which, taken in connexion with those of Davy, clearly indicate, that some chemical action has taken place during respiration, had also their influence.

If we compare the circumstance of carbonic acid existing so abundantly in the blood, as above described, with this view, we shall find them mutually confirm and explain each other. The 3½ per cent. of oxygen, received into the lungs with a portion ($\frac{1}{7}$) of its accompanying nitrogen, is absorbed by the internal surface of the lungs, (an extent more than equal to that of the whole human body, and which has lately been shown to possess almost unlimited powers of absorption,) just as the fumes of alcohol, æther, turpentine, or opium, are every day taken up by it, and conveyed into the circulating system. We do not stop to ask whether this is effected by the absorbent vessels or veins of the lungs, being satisfied that, whatever is the agent, the transfer is most rapid. Admitted into the blood, the greater part of this oxygen goes to form carbonic acid, which exists there so plentifully, and the remaining portion unites with the other elementary principles, to form combinations and products of which we shall perhaps remain for ever ignorant. Sir Everard Home has just taught us, that in the blood, the fixed air is the great agent of regeneration and nutrition; and its excess, analogous to all the other *media* employed by nature, is thrown off with the excretions, by the urine, by the sweat, and by the lungs. Hence the perpetual circulation of this gas, its ready formation in *tympanitis abdominalis*, in *pneumatosis spontanea*, and in lax debilitated human intestine. Hence its scarcity * in the blood of the cold-blooded animals, who respire imperfectly, and its universal exhalation from every animal organized substance.

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* Thomson's Chemistry, Vol. IV.

Let this view be illustrated by numbers. If the pulse beats four times for one respiration, as it commonly does, and the ventricle at each contraction exonerates itself of three ounces of blood; then a quantity equal to twelve ounces passes through the lungs during every respiration, containing within itself, according to Mr Brande's experiment, 24 cubic inches of carbonic acid gas, and perhaps in a state of slight combination, of which this acid is very susceptible. It ought, therefore, agreeably to our view, to be able to emit six cubic inches every pulsation, or twenty-four cubic inches in the time necessary to one respiration. But we know from experiment, that it scarcely emits two cubic inches of carbonic acid in that time; and therefore we ought to conclude, that the remainder (twenty-two inches) continue in the same state of combination in the *arterial* blood, as the whole complement of twenty-four cubic inches did in the venous, and that the addition of oxygen to the former, serves merely as a kind of ferment or stimulus, to the formation and exhalation of carbonic acid, whilst its entry into the pulmonary system by absorption, serves as a stimulus to elicit the ejection of carbonic acid by the common exhalents.

From this way of considering the subject it is easy to explain why alcohol † and fermented liquors diminish the quantity of carbonic acid. For these narcotic liquors, by reducing the sensibility, lessen the eliciting effect of the oxygen, while passing through the lungs—and thus produce a less liberal excretion of the former substance. It is only in this way that we can explain the curious fact, ascertained by Drs Fyfe and Prout, † of the quantity of carbonic acid being diminished while the system is under the operation of mercury; all the secretions being in that case greatly increased, and necessarily carrying off with them a portion of the carbonic acid, which should pass by the lungs. The same thing, and from a similar cause, takes place from nitric acid, and during a course of vegetable diet, because this matter has always a great tendency to combine with oxygen, and absorbs, in passing through the lungs, a part of that gas that should be employed in forming a *new proportion* of carbonic acid for the pulmonary excretion. The same theory also explains why most carbonic acid gas is formed at noon, less at midnight; the greatest and least excitability of the system (and consequently of the lungs) being found to exist respectively at these periods, which accordingly allows a proportionate elicitation of carbonic acid, by the oxygen inhaled.

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* Haller, Elem. Phys. I.

† Annals of Philosophy, II. 328.—IV. 331, 334.

The notion of hydrocarbon, as it has been called, thus appears not to be altogether without foundation; and the same process which forms carbonic acid, in all probability, at the same time, permits the elements of water to combine in the circulating fluid, afterwards to be exhaled from the surface of the lungs, as circumstances demand. The supposition is quite analogous to what takes place in vegetable decomposition; and the daily progress of the atomical chemistry in the analysis of animal products, renders it exceedingly probable that similar changes take place in these.

IV.

I. *A Practical Inquiry into the Causes of the frequent Failure of the Operations of Depression, and of the Extraction of the Cataract, as usually performed, with the Description of a Series of New and Improved Operations, by the practice of which, most of these causes of Failure may be avoided. Illustrated by Tables of the Comparative Success o the New and Old Modes of Practice.* By Sir WILLIAM ADAMS, Member of the Royal College of Surgeons in London, &c. &c. London, 1817. 8vo. Pp. 665.

II. *A Reply by* Sir WILLIAM ADAMS *to a Pamphlet recently published by* Dr VETCH, *upon the subject of the Egyptian Ophthalmia, and to other Productions of a similar character.* London, 1818. 8vo. Pp. 71.

III. *A Reply by* Sir WILLIAM ADAMS *to a recent Publication against him, purporting to be the "Report of the Committee of the London Infirmary, for curing Diseases of the Eye."* London, 1818. 8vo. Pp. 40.

Lettere del Professore Scarpa *al* Professore Maunoir *concernenti l'Opera del* Signior Adams *sulla Cateratta, e sulla pupilla artificiale.* 8vo. Milano, 1818.

We have no intention of trespassing upon the time of our readers, or of wasting the pages of our Journal, with any account of the numerous polemical effusions of Sir William Adams; nor do we think it necessary to contradict the various assertions or refute the various arguments by which he has laboured to diminish the effect of the opinion we have given of his merits and pretensions. *Magna est veritas et prevalebit.* The protection of those lords and ladies, generals and ministers, under whose sunshine he basks for a short season, will not avail him against the report and examinations of the soldiers who were selected as likely to receive benefit from skilful treatment, and were sent at a great expence from remote parts of the kingdom to be under Sir William's care, and who have returned, and are returning, with impaired and destroyed sight. It has, indeed, been stated in the daily prints, that a magnificent Ophthalmic Hospital for soldiers is erecting, or to be erected, in the Regent's Park, or somewhere else. But we cannot believe that there is any truth in the report, or that ministers could be guilty of such inconsistency, when they are practising the doubtful economy of reducing the second assistant-surgeons of the regiments to be preserved on the peace establishment; and when, as we know, and the public shall learn, ophthalmia has almost, if not entirely, disappeared from the army.

"My work of 1812, containing an explanation of my various operations, is in this Editor's hands, and defies his criticism." On the present occasion we shall comply with Sir William's wishes by taking up the gauntlet he has thus boldly thrown down; and we are happy in being able to present our readers with a most valuable criticism upon the improved edition of Sir William's great work, written by an ally, whose competency to judge cannot be questioned, and whose motives cannot be misrepresented.* It is sufficient to say, that the following article is written by SCARPA. It was first published in the Annali Universali for May 1818, and a copy of the sheets was transmitted to Mr Wishart of this city, to whom we are indebted for a perusal of the original, and for a faithful translation, which we have not ventured to abridge.

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* Professor Scarpa, in his private letter to Mr Wishart, dated 16th June 1818, accompanying his printed letters, speaks of his motives for writing them, which it is but candid to quote. "You will have remarked, in the recent work of Sir William Adams upon Cataract, that he criticises me in several places. This has induced me to write several letters to Professor Maunoir of Geneva, which have been published in the Journal of Milan. I esteem greatly the talents and ability of Sir William Adams; but I am of opinion, that the excess of his zeal for the progress of the oculistic art carries him beyond the bounds of truth and utility. In regard to *solid* cataract I do not believe there will be found

LETTER I.

Pavia, 17*th December* 1817.

DEAR FRIEND—As you appeared to me to be desirous of knowing the reflections which had occurred to me, in reading the recent work of Sir W. Adams,—" Practical Inquiry into the causes of the frequent failure of the operations of depression," &c. I send you them, submitting them most willingly to your impartial and candid judgment. In the first place, however, I beg you will allow me to detain you with a few words on a point which concerns me personally.

Sir W. Adams, although with expressions of politeness and civility, (p. 77.) refuses in this work, in the most formal and express terms, to grant me the merit, whatever it may be, of having been the *first* to practise and publish a regular operative process, much more advantageous than that employed by Pott for the cure of the *soft* crystalline and *capsular* cataracts, by means of the needle, founded on observations which I first made with regard to the dissolving action of the aqueous humour, being *considerably greater in the anterior chamber* than in the posterior, or in the bottom of the eye. Sir William pretends, in the most positive terms, that all that has been said or done, either new or useful on this point, ought to be ascribed exclusively to Pott.

The veneration which I cherish for the memory of this great surgeon, whom I had the good fortune to know intimately, would have dissuaded me from entering on this discussion. But as I am urged by the authority of your friendship to do it, I believe it may be of advantage to communicate to you some hints, not for the purpose of removing from me the charge of presumption, of which it is well known that I am incapable, but because it may perhaps one day contribute to give precision to the history of surgery, with regard to the operation of cataract by means of the needle.

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any one who, if he is able to depress it with facility, and by that means speedily to restore sight to the patient, would have recourse to two operations, both of them laborious and dangerous; the one to divide it into pieces, and the other to extract it by opening the cornea. In my first letter you will find some things which regard me personally. Mr Maunoir has been in London for two months, where he has, perhaps, published these letters, if so, I hope that Sir William Adams will see the just motives which have induced me to write them; if not, science will lose nothing by the discussion."

To speak precisely and consistently with truth on this subject, we must allow, in the first place, that the phenomenon of the dissolution and disappearance of the crystalline affected with cataract, as a sequel of the operation with the needle, was known long before Pott wrote about it, at a period when, even, the nature and precise seat of cataract were not sufficiently understood. Pott, in fact, did not conceal, that Read in 1706, and he might have said Bannister in 1622, had carefully recorded this singular occurrence after the operation of cataract with the needle. No one, however, in my opinion gave an account of this phenomenon with greater clearness and precision than Barbette, 23 years before Read, (Chirurg. Barbette, Genev. 1683, p. 49.) "Licet," he wrote, "cataracta non satis intra pupillæ regionem sit depressa, dummodo in particulas sit divisa, perfecta visio intra sex, aut octo septimanas, sæpissime redit, licet tota operatio absque ullo fructu peracta videatur, quod aliquoties experientia edoctus loquor."

From this passage you will perceive, that Pott was not only not the first who observed and described this phenomenon, but likewise that he in his practice did not perform, for the cure of the *soft* crystalline cataract, more than had been practised, on purpose, or accidentally, by Barbette, viz. the leaving *in situ* the soft crystalline cataract deprived of its capsule, and divided into several pieces, *dummodo in particulas sit divisa.* The merit of Pott, therefore, in all this affair, is reduced to having recalled *in a general way*, the attention of surgeons to the wonderful activity of the humours of the eye, in dissolving the substance of the lens deprived of its capsule, and in having, with the assistance of the anatomical and physiological knowledge of the present day, with regard to the wonderful activity of the absorbent lymphatic system, given the true and complete explanation of this phenomenon.

The researches of this, in many other respects, distinguished surgeon, did not extend beyond the limits now pointed out; and he did not even suspect the difference of the activity of the aqueous humour upon the substance of the crystalline, according as the particles of it were situated in the first, second, or third chambers of the eye. It appears that he thought that the powers of this menstruum were every where equal, whether the fragments slipped into the anterior chamber, or remained in the posterior or their natural situation. If I am not very much mistaken, I was the first who remarked that the fragments of the cataract are dissolved and disappear much more rapidly in the anterior chamber of the aqueous humour, than in the posterior, and that this takes place more slowly than any other where, in the vitreous humour. Even Sir William Adams allows me (p. 292) the precedence on this important point of discussion: "The Professor was the first, (as far as I am informed,) who remarked the greater rapidity with which the cataract becomes absorbed in the latter humour than in the former." And it was from the observations made by me that were derived, as a consequence, the fundamental precepts of the process of operating, first practised by me, for curing with the needle the *soft* crystalline and *capsular* cataract, very different from that taught by Pott.

It appears to me, that we are authorized to say, that, precisely from the want of this knowledge, Pott stopped at what had been already written by Barbette, and that, therefore, he did not bring, as he might have done, the operation for *soft* crystalline cataract to that degree of perfection of which it is susceptible; for, with regard to the *solid* cataract, this celebrated surgeon depressed it invariably into the vitreous humour, with success. There can be no doubt, that, if he had been acquainted with the great advantages which are obtained from making the fragments of the lens pass into the anterior chamber, he, as a general rule, would not have contented himself with leaving them in the posterior chamber, behind the iris, or, as he expresses it, in *their natural situation.*

The following is the passage to which I allude. "In order to render the fact still more clear, I have sometimes, when I found the cataract to be of the mixed kind, not attempted depression, but have contented myself with a free laceration of the capsula; and having turned the needle round with my finger and thumb, within the body of the crystalline, have left all the parts in their natural situation; in which cases I have hardly ever known them fail of dissolving so entirely as not to leave the smallest vestige of cataract. *In a few instances*, where I have had fair opportunity, I have pushed the firm part through the pupil, where it has always perfectly dissolved and disappeared."*

From this it clearly appears, as I have pointed out a little before, that Pott, as a general rule, did not do more for the cure with the needle of the *soft* or *caseous* crystalline cataract, than had been done (on purpose or accidentally) by Barbette and Read.

But Sir William Adams compares Mr Pott's mode of operating on *soft* cataract with the operative process which I have employed and described. To rupture or separate the *soft caseous* cataract, I abstain from turning round the needle between my finger and thumb, because it is not possible to do this with a free and moveable lens, without the danger of rubbing the posterior surface of the iris. On the contrary, on the capsule being ruptured, I do nothing more than, with the point, push very gently the lens towards the pupil, by which pressure the *caseous* substance of the cataract breaks, and passes in fragments into the anterior chamber. Sir William Adams will perceive, that, in doing this, I have not restricted myself, as Pott did, to wait for a *fair opportunity* for pushing the fragments of the soft crystalline cataract into the anterior chamber, but that I have always propelled them into it ; and I have directed that this should always be done, this *favourable opportunity* being never awanting, when the lens is deprived of its capsule, and so soft as to be reducible into pieces by moderate pressure of the needle, which pushes it forward.

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* Mr Pott adds in a note, "I should be sorry to have it inferred from hence, that I would recommend the passing the opaque crystalline through the pupil far from it; I think it wrong, as it is apt to produce one of the most frequent inconveniences attending the operation of extraction—an irregularity of the pupil. I only meant to prove the fact of dissolution of the cataract in such situations, and that it will not cause that pain and trouble which it is so positively said to do."—*Editor.*

What will surprise you most in this subject is, that while Sir William excludes me from every claim, he disapproves highly of the conduct of Pott and of those who followed wholly, or in part, his practice, such as Hey and Saunders, for the cure, with the needle, of the soft crystalline cataract, not perceiving, that in that manner he grants to me what he had previously denied. Observe how he expresses himself, (p. 356.) " I object to the practice of leaving the opaque lens to be absorbed *in situ*, after lacerating the capsule, and loosening its texture with the needle, because, in consequence of the latter part of the operation, an uneven pressure is made by the broken fragments of that body against the posterior surface of the iris, frequently giving rise to severe and dangerous inflammation, besides which, solution and absorption of them are by no means effected so rapidly, as when they are placed in the anterior chamber."

Here you already anticipate what I am going to say ; for if the operative process which Sir William approves of, viz. not to leave the fragments of the *soft* cataract *in situ*, but to push them in succession into the anterior chamber of the aqueous humour, is the same process in fact as was for the first time practised and described by me ; and if this process of mine is different, as it certainly is, from that of Pott, which Sir William disapproves of, it is evident, that it is not to Pott, but to me, that the merit belongs of having employed, for the *first* time, a regular rational process, more advantageous than that of Pott, for curing, with the needle, the *soft caseous* crystalline cataract.

You will search the works of Pott in vain for a rational and certain rule for curing with the needle the *capsular* or *membranous* cataract. He has only left, on this important point, the following observation, that is, of having seen the capsular cataract disappear sometimes in one, two, or sometimes four weeks, and sometimes to continue so long as to require the re-application of the instrument. (p. 153.) From this silence of the author on this mode of reducing the opaque capsule into pieces, it appears, that we may infer that he thought that the capsule ought to be reduced into small pieces by the same method as he employed for breaking down the *soft* lens, that is, fixing the needle in the capsule, and turning it between the finger and thumb. But experience has demonstrated to me, that this turning of the straight needle is not sufficient, either for rupturing or reducing into minute fragments the *soft* opaque capsule, and still less the *hard*. It is necessary to pull it in various directions, and repeatedly to *scratch* it, to use the expression, in order to reduce it into small particles, and detach it from the ciliary circle. To effect this, I have found, that my *curved* needle is much preferable to the *straight* needle; and, indeed, Sir William Adams makes use of a similar needle, in circumstances not dissimilar from the above mentioned. The leaving the detached and divided capsule, if its division could be effected with a straight needle, in the posterior chamber, as was done by Pott, could not but greatly retard the cure; for, in that situation, the dissolution of the membranous flakes is very slow, slower even than that of the fragments of the *soft* crystalline; and it appears as if these membranous flakes, before beginning to be dissolved, swell and become firmly attached to the margin of the pupil, which they close completely for a long time, if they are not pushed by another operation into the anterior chamber of the aqueous humour. All these circumstances, regarding the more effectual cure of the *capsular membranous* cataract, primary or secondary, which are not found in Pott's writings, I have distinctly, and, for the first time, pointed out. And how could Sir William Adams be ignorant of this?

I suspect that Sir William Adams has confounded operative *method*, or the *general principle* of the operation, with *operative process*. As to the operative method, or *general idea* of curing with the needle, a *soft* or *capsular* cataract, by removing it from the axis of vision, and breaking it into minute fragments, in order that it may be easily dissolved by the aqueous humour, and afterwards absorbed, I agree with Sir William Adams that I have no pretensions, nor Pott either; as that was known long before he wrote. But with regard to a regular, rational, complete *operative process*, no one, and least of all Sir William Adams, can deny the preference to my mode of operating, compared with that of Pott. This truth results, besides the arguments I have hitherto advanced in proof of it, from the effects which my work, on its first appearance, produced in the minds of distinguished surgeons. The works of Pott had been for many years in the hands of the profession. Nevertheless the favourers of *extraction* continued to adduce and support their objections against *depression*, persisting in saying that it would never be possible to cure with the needle a *soft* crystalline or *capsular* cataract. It was not until the appearance of my work on the diseases of the eye, that the language was changed; at which period several celebrated surgeons abandoned extraction, to adopt the practice of removing the cataract with the needle. On this subject Leveillé thus expresses himself:—" Il y a dix ans, lorsqu' j'ai publié à Paris, cette methode d'operer la cataracte selon *Scarpa*. Les praticiens fort exercés à l'extraction, ont traité de fabuleux, et d'imaginaire, tout le bien que j'avais eté fondé à en dire. Mais comme je ne parlais que d'après des faits, il a bien fallu m'en opposer des contradictoirs. Qu'est il arrivé? Le temps et l'experience ont jugé la cause en faveur de la méthode de *Scarpa* a tel point, qu' aujourd'hui des chirurgiens de premier ordre, parmi lesquels je me plais de nommer MM. *Dupuis* et *Dupuytren*, adoptent cette methode de preference à celle par extraction." (*Nouvelle Doctrine*, Vol. IV.

p. 360.) And here I embrace the opportunity of pointing out that Leveillé, in his interesting Memoir, soon after the translation of my work, *Methode d'opérer la cataracte par deplacement et par absorption*, has anticipated Sir William Adams in giving to the operation of cataract by means of the needle, the denomination of *absorbent practice*. (p. 293.)

I have great reason to believe, that, at the same period, or after the appearance of my work, Sir William Adams himself abandoned the practice of Pott, Hey, and Saunders, to approach to mine as may be inferred from the following passage: (p. 357)—"Such was Mr Saunders's practice while I was with him, and which I pursued for some time after I first settled in Exeter; I, however, often found the inflammation so very severe, and the necessity of repeating the operation so frequent, that I shortly abandoned it altogether in favour of that which I now exclusively practise. By this change of practice, and by the use of the needle which I employ, inflammation rarely follows. From the greater solvent power, also, of the aqueous humour in the anterior chamber, I am enabled, by placing the fragments of the cataract there, to effect their removal in half the time; and by one, or, at most, two operations, while formerly, when the cataract was left to be absorbed *in situ*, it was frequently necessary to operate four or five times to effect the same object." This period is posterior to the publication of my work on the diseases of the eye.

I return to mention to you, my claim does not turn upon the *general principle* of the *operation by absorption*. I would show myself to be completely ignorant of the history of our art in making such an assertion. I am sure that Sir William Adams could not have done me the injury to entertain such a supposition. The discussion, therefore, is with regard to the preference of my *operative process* to that of Pott, for the cure with the needle of the *soft* crystalline and *capsular* cataract, a preference denied to me by Sir William Adams, but proved by the facts above detailed, and by the very change of practice of Sir William posterior to the publication of my work.

I have said enough, perhaps too much, on this subject. In my next letter I will write to you my sentiments with regard to the *new* operation for the cure of the *solid* cataract.—I am, &c.

LETTER II.

Pavia, 27th December 1817.

After reading a few pages of Chapter II. and IV. Section I. of the recent work of Sir William Adams, you will completely perceive the difference between the common mode of curing with the needle the *solid* crystalline cataract, and the *new* operation proposed and performed by him for the same purpose.

I, as well as other depressors of cataract, sometimes meet with a *solid* lens, of which I can lay hold with my *curved* needle, and remove it with facility from the axis of vision, and with equal facility depress it into the vitreous humour ; and I have no reason to be dissatisfied with this practice. Sir William Adams pretends that this mode of operating has frequently, not to say in most cases, failed ; and that, to obtain with certainty the cure of a *solid* cataract, it is proper to break it into pieces with his straight needle, cutting on both edges, and to push the fragments in one mass, or in several pieces, into the anterior chamber of the aqueous humour. Whenever the lens is so *hard* as to resist the division with the needle, it is proper to lodge it entire in the anterior chamber, to be immediately after extracted through an incision of the cornea proportioned to the size of the lens pushed forward ; so that the *new* operation, in a case of very hard cataract, or one having a nucleus resistent to the dissolution in the aqueous humour, is a compound of the operation of depression and of extraction. The reasons on which Sir William rests the propriety, necessity, and utility of his *new* method, are the following.

1. Because the vitreous humour, in the healthy state, possesses a force of elasticity or repulsion, (propelling power,) so strong, when it is compressed, as to make the hard lens reascend in most cases, which it will be vain to attempt to depress into it.

2. Because the *solid* cataract does not always remain depressed unless the vitreous humour is disorganized and fluid like water.

3. Because when the vitreous humour is morbidly organized and fluid, the solid cataract depressed into the bottom of the eye, resting upon the retina, constantly occasions most violent pain, severe inflammation, and, finally, amaurosis.

If these reasons advanced by Sir William were founded in truth, and confirmed by experience, there is no doubt that it would be proper to desist wholly from the depression of the *solid* lens, in order not to expose, in most cases, patients to such severe accidents, as well as to be disappointed of their hopes; and, therefore, to attempt every way to remove from the eye this species of cataract, whenever it was partially or entirely resistent to division, and to the solvent power of the aqueous humour. But, before determining this point, it appears to me that the above-mentioned reasons ought to be subjected to a strict examination.

To demonstrate the considerable force of elasticity which the vitreous humour possesses in the healthy state, Sir William Adams adduces (p. 88) as a sole and principal proof, that this pellucid body, removed from the eye and placed on a table and slightly pressed, rolls about without separation ; farther, that after the extraction of the cataract, if the muscles of the eye become affected with spasm, the vitreous humour presents itself between the lips of the incision of the cornea, on examination, is felt quite firm to the touch. He relates the case of a lady (p. 89) in whom, after the extraction of a cataract, while passing from the place of operation to her bed, a large portion of the vitreous humour was forced out by the violence of the exertion of the muscles of the eye. If you remark also, that, in another place, (p. 93,) he says that the *sudden percussion* of the muscles of the eye is not capable of lengthening its longitudinal axis the one eight hundredth part of an inch, one would almost be led to infer that the vitreous humour attempts to pass out of the eye by its own inherent elasticity. I do not think it necessary to offer a single remark with regard to the futility of these two arguments intended by the author to prove his assumption. The vitreous humour receives its pretended great elastic power, according to Sir William, from the membrane surrounding it, endowed, as he asserts, with considerable tenacity and flexibility, but which no anatomist has hitherto remarked. Every anatomist, on the contrary, knows that this membrane is so delicate and fine as to be justly named *aranea*, because it breaks with great facility. Deprive, I beseech you, the eye-ball for a few lines, of the sclerotic and choroid coats, so as to allow a corresponding portion of the vitreous humour to present itself; rest upon it by the handle the convex part of my needle, or a blunt probe, and make a slight pressure, as you would do in depressing the cataract; and you will perceive, on the slightest motion of the convexity of the needle or of the probe, the tunica *aranea* break, just like a spider's web. Repeat the same experiment, applying a solid crystalline, and trying to depress it, you will find that, on the slightest pressure, the *hyaloid* membrane opens and allows the lens to enter into the cavernous substance of the vitreous humour. Repeat the same experiment, removing the cornea and iris; then open the capsule and depress the *solid* crystalline, pushing it from above downwards, and from before backwards, in the arch of a circle, you will ascertain how slight pressure is required for rupturing the *hyaloidea* or tunica *aranea*. Do not fail to observe that, with this slight movement, you rupture, at the same time with the membrane of the vitreous humour, the posterior circumference of the capsule; which, you know, is much finer than the anterior. On this point the object of nature appears to me to be deserving of your attention, in combining this gradation of the density of the internal pellucid membranes of the eye, in giving greater density to the anterior capsule, less to the posterior, and still less to the *hyaloidea*, immediately behind the posterior capsule, and the least of all to the same membrane of the vitreous humour at the bottom of the eye.

A very small degree of pressure is sufficient, even in the living subject, and in the state of greatest consistence of the vitreous humour, to lacerate this fine membrane, and depress the *solid* lens into the bottom of the eye; provided you give to the needle that motion in the arch of a circle which I have recommended. In every case where this motion is properly performed, the depressed lens does not reascend, and, in my opinion, it cannot reascend; both because its specific gravity is greater than that of an equal portion of the vitreous humour, and because the depressed lens is pushed under the intimate cellular structure of this body; and, lastly, because, whatever degree of elasticity we may wish to ascribe to the fine tunica *aranea* of the vitreous humour, it cannot react in any way at the place where it has been ruptured. In the dead body of an old woman affected with *solid* cataract, a few days ago, depression was performed with my needle in the usual way; then the operated eye was frozen in its situation in the orbit, in order that no change might take place in the internal part of this organ besides those which had been expressly effected with the needle, and with great slightness of hand. On opening the eye-ball the lens was found lodged in the substance of the vitreous humour, behind and towards the temple; consequently there could be no doubt, that, by this slight pressure made with the needle, and by the lens on the *hyaloidea*, that delicate membrane had been ruptured.

However slight the resistance, as it is in fact, which the vitreous opposes to the pressure, I am, however, of opinion, that if the lens freed from its capsule is only pressed slightly from above downwards in a line parallel to the posterior surface of the iris, on withdrawing the needle the cataract may reascend. But this can never happen if this membrane is ruptured, and the *hard* lens plunged into the substance of the vitreous humour. If the reascending of the *solid* lens regularly and properly depressed were so frequent, as Sir William pretends, it is not probable that, from the days of Celsus till now, the great number of failures would have been concealed, or confined to a small number of cases, and these even not sufficiently authenticated. Nor is it credible that, if this had happened, St Yves, Maitre-Jean, Brisseau, Cheselden, Sharp, Benevoli, and innumerable other ancient and modern surgeons, would have acquired by means of this operation the reputation of excellent oculists. On this point it appears to me to be worthy of attention, that, prior to the period when surgeons began to fix their attention upon the *capsular* and *secondary membranous* cataracts after the depression of the solid lens, some cases of reascended crystalline cataract have been mentioned. After this period there has been no mention, at least in most cases, but of *membranous* or *capsular* cataract, consequent of depression. By retracing these observations it will not be difficult to detect the mistake into which those have fallen who have made mention of *solid* cataracts risen up, or at least to reduce the cases reported by them to a very small number, and a few rare examples.

Sir William Adams writes (p. 97) that he was informed by Surgeon Este that he had seen the cataract reascend in three cases operated on by me; but when depressed anew, it did not again rise, and that my mode of operating was generally successful. Whether this young man, who, soon after commencing his first anatomical and surgical studies in this university, left it, has seen *secondary capsular* cataract, or a reascended opaque *hard* lens, I will not venture to assert. I do not recollect that any *solid* cataract depressed by me has reascended; nor does any of the numerous pupils recollect such a thing; although *secondary* capsular cataracts occur. But, supposing even that this inconvenience had taken place in the cases alluded to; (as there is no reason to doubt that in the three patients I operated on, the vitreous humour possessed the full vigour of its *propelling power*;) nevertheless, as soon as the lens was depressed properly a second time, that is, by lacerating the hyaloid membrane, the depressed lens did not reascend. This fact is a strong argument against the opinion of our author. I indeed consider as mere fables (p. 94) the accounts of *solid* crystalline cataracts that have returned to their natural situation after three, four, or even ten years after they had been carefully depressed into the bottom of the eye. I would go a step farther, and I do not hesitate to assert, that a large fragment of a *hard capsule,* although much lighter than an equal portion of crystalline, if it resists the grasp and pressure of the needle, may be immersed into the centre of the vitreous humour, without any fear of its force of elasticity repelling it towards the pupil.

Besides the considerable elasticity which Sir W. Adams ascribes to the fine and friable membrane of the vitreous humour, and which he does not hestitate to compare to a *pressed spring* (p. 88) in the act of depressing the *solid* lens towards the bottom of the eye ; he has perceived the difficulty that whenever the cataract is *hard*, it is depressed with the greatest facility ; not so the *mixed* cataract, on account of a portion of it, in the course it has to pass through, slipping from the needle and receding towards the pupil. On which point, instead of untying the knot, he has thought it better to cut it ; saying, that, when so great facility is met with in depressing a *solid* cataract to the bottom of the eye, it is because the vitreous humour is disorganized and dissolved into water ; in consequence of which, by merely resting the needle on it, the capsule is detached from the ciliary processes, (he ought to say from the *zona ciliaris,* because the ciliary processes have no connection whatever, either with the capsule or lens,) and is plunged with the lens into the bottom of the eye, as, says he, a *stone thrown into water.* Sir William Adams does not hesitate to assert, what you will find still more singular, that this degeneration and fusion of the vitreous humour is a very frequent morbid state of the eye, especially in old persons ; for, he says, the partial or total dissolution of the vitreous humour follows the ratio of the age. (p. 105,327.)

In the long series of years in which I filled the anatomical chair, I have never, in dissection, met with this state, which ought to be very obvious to me as well as to every other person. Nor do I know, that any other anatomists, more careful and more engaged in the anatomy of the eye, have remarked any similar occurrence with regard to the vitreous humour. Within these few weeks, since reading Sir William Adams's work, I have made at least forty eyes be examined, of persons who have died between sixty and eighty years of age, and in not one was the vitreous humour found partially or wholly disorganized. It was merely remarked, that the vitreous humour in old people is less transparent than in young, and that in the former it acquires a slight yellow tinge.

No one will deny the possibility of a cataract being complicated with disorganization of the vitreous humour, but this rare combination cannot serve as a general rule, and still less can it be called frequent. The collectors of anatomico-pathological observations do not mention it. In the last century, and even prior to that when the greatest care was employed in the examination, in the dead subject of the eyes of cataract patients, in order to determine the true nature and seat of this disease, no one has recorded that cataract is frequently complicated with fusion of the vitreous humour. *Rolfink* was the first, as far as I know, to mention this disease of the vitreous humour, or dissolution of the vitreous humour into water, without any vestige of crystalline, in the body of an amaurotic patient, (*Dissert. Anat. lib.* 1. c. xiii. p. 180. *anno* 1656,) which seemed so strange to *Gaspar Hoffman,* that he wrote, such a thing could not be *in rerum natura.* A long time afterwards *Brisseau* relates, that he met with the vitreous humour dissolved in an atrophic eye ; then Petit, that he had seen this body dissolved in an eye in which the crystalline was opaque and hard ; afterwards Heister met with the vitreous humour fused in an amaurotic eye. I do not find any other facts recorded about this period ; and the small number of these pathological observations is a proof of the extreme rarity, at least, of cases of cataract complicated with dissolution of the vitreous humour. Morgagni, in his numerous anatomico-pathological investigations, wrote, that he had only met with disorganziation of the vitreous humour, in two instances, in animals ; and when he met with it, he always found this disease of the vitreous humour complicated, not with cataract, but a diseased state of the coats of the eye, especially of the sclerotic coat. He further adds, that the change of colour of the vitreous humour, is not a sure symptom of its dissolution ; for the vitreous humour is sometimes found of an unnatural colour, although its consistence be natural. The oscillatory motion of the iris is not a certain mark of degeneration of the vitreous humour, as some writers have pretended. The fusion of this body was found at periods posterior to the above mentioned, to be generally conjoined with dropsy of the eye, sometimes without opacity, sometimes with opacity of the crystalline, in the advanced stage of the disease. But neither dropsy of the eye. nor diseases of the sclerotic coat, have any thing in common with the doctrine of Sir William Adams, and still less with the eye affected with cataract, when possessed of all the requisites favourable for operation.

Sir William Adams tells us, (p. 106,) that of 31 Greenwich pensioners operated on by him, in no less than 14 cases the vitreous humour was in part, or wholly disorganized. But did he dissect the eyes of these 14 patients ? From what symptoms, then, did he ascertain that in these individuals the vitreous humour was partly or wholly converted into water ? Perhaps from having seen more limpid fluid than usual flow along the double edged needle, or iris scalpel for the incision of the iris ? But this is not a certain mark of dissolution of the vitreous humour ; for, in pushing the double-edged needle, or the small knife, through the sclerotic coat, between the ciliary processes and ciliary circle, and then from the posterior into the anterior chamber of the aqueous humour, if this humour is in great quantity in the two chambers, as not unfrequently happens, and if the operator is obliged to make many and various movements backwards and forwards to cut in pieces the *hard* lens, or to divide the iris transversely with repeated small cuts, moving the knife in the manner of a saw, it would be easy for a great deal of the aqueous humour to escape out of the eye, along the blade of the instrument, and excite the belief, that that limpid fluid is chiefly the dissolved vitreous humour, when the greater part of it is aqueous. I say the greater part, for, as the cutting needle passes from the sclerotic coat into the anterior chamber, it necessarily divides and opens a portion of the cells of the vitreous humour, from which a portion of fluid is effused and passes out with the aqueous humour of the two chambers along the needle.

The opinion, therefore, of our author, with regard to the frequency of the disorganization of the vitreous humour, especially in persons advanced in years, and with regard to the frequent complication of cataract with the dissolution of this humour into a watery fluid, is merely an *hypothesis*, for which, it appears, he had occasion, to give weight to the third objection which he makes against the depression of the *solid* cataract into the bottom of the vitreous humour. To fortify this objection, he has drawn a frightful picture, copied in a great measure from *Wenzel*, of the very melancholy consequences which he supposes may be ascribed to the pressure and friction of the *hard lens* on the denuded retina; such as most acute pain, violent inflammation, and ultimately amaurosis. And this picture is the more horrible to the young student, that Sir William Adams does not furnish him with marks to know, if the cataract is complicated or not with this disease of the vitreous humour, or rather, he adds, that this disorganization of the vitreous humour sometimes comes on after the operation, performed in the best manner, in consequence of which the *solid* lens, being long of being dissolved and absorbed, it follows that, at a certain interval after the operation, the terrible symptoms above mentioned appear, and, to sum up the misery, amaurosis ensues.

You will have had occasion repeatedly to remark, that every hypothesis, however extraordinary it may appear, always involves some truth, but this truth is either too much generalized or perverted. The hypothesis of Sir William Adams, as every other, has some truth in it. For it happens in some very rare case, after the most easy and speedy depression of the lens, that the patient is attacked with pain and *internal* inflammation of the eye, (not, however, so violent as the author speaks of,) in consequence of which, after these symptoms are subdued by proper remedies, the pupil is found perfectly clear, but more dilated than natural, and immoveable, and the patient is evidently become amaurotic. This distressing accident has only happened to me in a single instance during my long practice. I know, however, that it has likewise happened to other very experienced practitioners, but, as I have mentioned, very rarely. It appears to me to be probably this very rare occurrence which Sir William has assumed as a model for generalization, or at least for calling it frequent. With regard to which it is proper to observe, that the operation of extraction is likewise liable, in some very rare instances, to the same accident; and, therefore, that it cannot be ascribed to the pressure of the hard lens upon the retina. It is said, that, when amaurosis occurs after extraction, it depends upon the stretching of the iris by the lens, in its passage through the small pupil, or the too great discharge of the other humours along with the lens, with flaccidity of the membranes of the eye. But what connection has the iris with the retina? or by what means? and on how many other occasions do we see flaccidity of the membranes of the eye, from too great effusion of the humours, without amaurosis being the consequence? Observe, that amaurosis sometimes follows immediately after a blow, with laceration of the superciliary nerve of the fifth pair. What relation has this nervous twig with the retina or optic nerve? No one can precisely say. There is, therefore, a secret consent between this nerve, distinct from the eye and the immediate organ of vision. And why may not a similar consent exist between the differentother parts composing the eyeball and the retina, which consent may be violently excited, and only under very rare circumstances, during, or after depression or extraction, in patients of extreme sensibility, and from a certain kind of injury done to the membranes of the eye, apparently of little consequence? I am not much disposed to hazard conjectures; but if I were forced to declare in favour of any, in order to give a probable explanation of this very rare phenomenon, I would have recourse to this, rather than to the pressure of the lens plunged into the bottom of the eye, and pressing closely on the soft substance of the retina.

Lastly, Because, as I have mentioned, in some very rare cases, amaurosis occurs after the depression of the *solid* lens, (which also happens very rarely after extraction of the opaque crystalline,) Sir William Adams was not authorized to generalize this fact, and, passing from hypothesis to hypothesis, to ascribe the cause to the frequent complication of cataract with fusion of the vitreous humour, and further to the pressure of the *hard* lens on the tender, immediate organ of vision.

I reserve to my next letter to speak of the *new* operation for the cure of the *solid* cataract.—I am, &c.

(To be concluded in the next Number.)

V.

Medical Report of the House of Recovery and Fever Hospital in Cork Street, Dublin. By F. Barker, M. D. Honorary Fellow of the King's and Queen's College of Physicians; Professor of Chemistry in Trin. Col. Dublin; and senior Physician to the Hospital. Dublin, 1818. 8vo. Pp. 80.

We have already had occasion, more than once, to express our sense of the value of the admirable institution from whence this document has been issued; which, since its opening in May 1804, has received within its walls more than 27,000 patients labouring under continued fever; and has been conducted at all times, but especially during the epidemic now prevalent in Ireland, with such wisdom and benevolence as to reflect the highest credit upon the managers. The present report, by the senior physician, who has held his office ever since the establishment of the institution, embraces the last most eventful year in the history of fever. It is, in itself, a tract of very considerable value, and at the present moment is entitled to more than ordinary attention.

Notwithstanding the constant agency of the hospital in Cork Street, and of some other excellent institutions in Dublin, the increase of contagious fever was, in the year 1810, so great, as to convince the managers, that the causes of the disease were beyond the controlling power of any such institutions, however extended; and the malady continued to increase from that period to 1815, when the applications at the hospitals became so frequent, that new buildings were thought necessary at Cork Street; and new wards were opened also at the House of Industry. But fever, nevertheless, maintained its ground, until the autumn of 1817, when the Epidemic which, for more than a year, had prevailed in most parts of Ireland, arrived at the metropolis. In spite of every measure employed to check its progress, "the disease," Dr Barker adds, "has made steady advances; and patients now enter the hospitals (the report is dated 1st October 1818) at the rate of at least 2000 monthly."

The circumstances which preceded this last dreadful invasion, are very well detailed by Dr Barker; and we have reason to know, that the picture which he has given of the sufferings of the poor in Ireland, during these last two years, is not at all exaggerated. In 1816, the crops had failed, from the unusual cold and moisture of the seasons; rain having fallen in great abundance during the summer, and the mean temperature of the spring, summer, and autumn, having been nearly 3½ degrees below that of the same period of the preceding year. In the vicinity of London, in France, and over a great part of the northern hemisphere, a like unfavourable state of the weather appears to have prevailed. The year 1817, also, was cold and moist, the harvest consequently was very much affected, and the grain and potatoes were both scanty and bad. The consequences were, a scarcity, amounting, in some parts of Ireland, almost to famine, which was at its height about the midsummer of 1817; bread bearing, at that period, more than twice, and potatoes, in many places, more than three times their usual prices. The sufferings of the poor, under these circumstances, particularly in the north of Ireland, were dreadful; they were frequently seen gathering wild esculent plants to allay the pains of hunger; and some unfortunate creatures appear to have died of actual want. Mendicants now crowded from all quarters into Dublin, and the increase of fever, in places remote from the city, was truly formidable. But, it is remarkable, that the disease did not arrive at the metropolis till some months afterwards, when the pressure of the scarcity had ceased; so that, when it commenced in Dublin, the prices of bread and potatoes did not much exceed their usual rate. The disease began in the small neighbouring towns to the north and west of Dublin, but did not reach the city itself, till about the middle of September 1817, at which period, Dr Barker shows, that a simultaneous increase of fever took place also at Cork, Waterford, and Edinburgh; and, he states it as worthy of observation, that, during an epidemic fever, which prevailed in the south of Ireland in 1801 and 1802, the hospital admissions for fever increased, at Manchester, to more than 54, double the average number of five preceding years; and that the epidemic at Gibraltar, in 1804, 1810, and 1813, also commenced in autumn.

The disease, which thus appeared, whilst by some persons it was alleged to be neither dangerous nor contagious, and by others was denied to exist altogether, excited very general alarm, and various measures of prudence and humanity were adopted to meet the danger. New beds were opened at several different hospitals for the reception of patients. The range of the Fever Hospital was extended, and all applicants were admitted on their first application; the symptoms being so well known to the poor, that very few mistakes occurred. On the approach of summer, the numbers still increased, and this augmentation had continued up to the date of this publication, at which period more than 1230 beds were insufficient for all the applicants at the hospitals; and Dr Barker considers it as very doubtful, whether the frequency of fever has yet arrived at its maximum. He did not observe, that moisture had any remarkable share in the progress of the epidemic; but he conceives, that fever is increased in general during the summer and autumnal months; and that a low temperature will not prevent its spreading, as was proved at Altona during the winter of 1813-14, where fever continued to prevail at a temperature of 34° below the freezing point. †

The disease, of which we have now sketched the progress, was typhus of the ordinary character. It was decidedly contagious; but the latest period, after infection, was not easily ascertained. In several instances, it amounted to some weeks, and the length of this interval favoured the opinion, that the complaint did not originate in contagion. A tailor, who had worked sitting on an infected blanket, was attacked with fever on the 4th day of his being so occupied. At first, the disease appeared much less contagious than subsequently; a fact which accords with the observations of Dr Edmonston at Newcastle in 1817, and with those of Dr Russell respecting the plague at Aleppo. Dr Barker has given tables, exhibiting the ages of patients admitted at the Fever Hospital, from whence it appears, that persons between the ages of 10 and 12 constituted the largest number; the admissions, according to the ages, being in the proportion of the following numbers, viz.

371	10 to 20 years of age.
318	20 to 30
127	30 to 40
91	0 to 10
60	40 to 50
30	50 and upwards.

It is to be observed, however, that no inferences can correctly be deduced from the records of an hospital, respecting the prevalence of the disease at different ages in society at large.

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* The great epidemic, which commenced at Dublin in the autumn of 1740, did not terminate till the beginning of winter 1742.

† Steinheim ueber den typhus, im Jahr 1814, in Altona.

From a table of the admissions and deaths,* during a period of nearly 16 months, from 5th January 1817 to 30th April 1818, it appears, that, at the commencement of the epidemic, in September 1817, men were the chief sufferers, both as to frequency and mortality. The total number of admissions was 5732; males 2883; females 2849. The deaths were, of males, 179, or one in 16; of females, 141, or one in 20; the average of both being one in 18. The fever pervaded all ranks in society; and, at first, apparently, in equal proportion. Shopkeepers, it was observed, were frequently seized with it, probably from beggars crowding about their doors, and pawnbrokers were very often affected.

During the prevalence of the disorder, other acute diseases appeared to be suspended, as observed during great epidemics in other places. A female patient was received, by Dr Barker, in Sir Patrick Dunn's hospital, who laboured under small-pox. A few days after her dismissal, her sister, who had never had that disease, and who had slept in the same bed, was attacked with febrile symptoms, and small-pox was naturally expected to appear; but the disease proved to be typhus under its usual form.

The symptoms of the epidemic, in general, afforded no great deviation from the usual course of fever; cases, however, were frequent, which resembled the *fievre ataxique* of French authors.

"For example, the pulse, either much slower or more frequent than is usually observed in fever, and this, at an early period of the disease, not indicated by other symptoms. The sensorium greatly affected, without great deviation from the appearances of health; in other respects, the body covered with petechiæ, other symptoms mild, and *vice versa,* the heat of the skin almost natural, whilst other febrile symptoms were present in a high degree, and the tongue clean and moist during a very severe illness. Irregular cases of this kind were most common at the commencement of the epidemic, and often terminated fatally. They were also most numerous among male patients, and persons accustomed to better living than the lower classes generally enjoy in this country. With the occurrence, more frequently than usual, of irregularity in the symptoms of fever, the frequency of petechial eruptions was also remarkable. These were exemplified in persons of all ages," (in many children, even under the age of four or five years,) "and that peculiar form of petechiæ, in which the eruption resembles the measles, occurred much oftener than heretofore. This efflorescence, more frequently than any other kind of petechiæ, is attended by symptoms of danger, more particularly when it is dark-coloured. This appearance, Dr Barker adds, is by no means of recent date. It was observed by Sydenham, and by Dr Rogers of Cork, in 1731; Huxham has described it in 1764; and Dr Gottwald, in his account of the plague at Dantzic, in 1813, describes a petechial eruption, resembling the measles, and spreading over the body. This eruption generally makes its appearance between the 5th and 6th days, inclusive, of the fever. It is accompanied uniformly by suffusion of the eyes, the conjunctiva being affected in a manner similar to that of the surface of the body, as happens in measles and scarlatina. It presents itself with very different degrees of distinctness; sometimes so faintly, as to be scarcely discernible, except on close inspection. On such occasions, suffusion of the eyes is a pretty certain indication of its presence. It is generally attended by stupor, or delirium, in various degrees. In some cases, it was so much elevated, as to be distinctly felt on passing the finger over the surface. The probable event of the case cannot be inferred from this eruption; I have seen it begin to disappear when the severe symptoms increased; though its fading is, I believe, generally attendant on convalescence. From the mere appearance of the eruption, it would be sometimes difficult to distinguish between it and the measles; but its not occupying the face in the same degree as the measles, particularly at its commencement, the history of the mode of attack, and the concomitant symptoms, sufficiently distinguish it from that eruption. It does not disappear on pressure. When the eruption appeared in this form, resembling measles, in a patient of the better class of society, this disease was generally dangerous; the same remark has been made in other places.* I have, for some time, entertained the opinion," Dr Barker adds, "that sufferers from fever, attended with this eruption, if they are not altogether secured by it from a second attack, are not at least so liable to it as those who have had a fever of the ordinary kind. Though I have frequently made the inquiry, I have not found a patient in whom this symptom was distinct, who had suffered from the same fever on any former occasion."

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* This table is judiciously divided into periods of 10 days, so that, by reading the last figure of any period as a decimal, we have the *daily* rate of admission and death.

Another peculiarity in this epidemic, was the occurrence of a livid or purple colour of the extremities, not accompanied by coldness, but mostly attended with great disturbance of the sensorial functions. It was a formidable, but not a mortal symptom, and was observed in severe cases only. This symptom was common in the fever of our army in Spain, during the winter of 1812, a fact which seems to point to the origin of the present epidemic. Cases of determination to the brain, with local inflammations in some external part of the head, were also frequent; and, in a few instances, termination in mania was observed, especially in summer. Vertigo was frequent during convalescence. Pectoral complaints were rare. Yellowness of the skin and adnata were not unfrequent, but did not indicate danger. Relapses, sometimes repeated, were frequent, after slight attacks of the disease.

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* It was observed at Altona, that in every case among the better ranks, originating from infection, petechiæ occurred, and patients of this description sunk more rapidly than others.

The progress of the symptoms was not regular, and the fever did not appear to decline, among the better ranks, on any fixed day. The great pressure of business prevented accurate observation upon this head at the hospital. As the summer advanced, cases lasting only four or five days were frequent; a circumstance observed also in the epidemic of 1801. In 1807, Dr Barker had recourse, for the purpose of determining the question of critical days, to a daily register of the symptoms, kept with great regularity in a tabular form;* and from these documents, kept for a number of years by himself and his late colleague, Dr Gamble, he was enabled to deduce the inferences already published in a former report; and, he now adds,

> "I am not disposed to insist much on the doctrine of critical days, as in many cases, no distinct crisis is discoverable. But, I firmly believe, that many febrile diseases have a disposition to terminate on certain days, dating from their commencement; and, if observed accurately, the regularity will prove to be much greater than on a hasty view might appear probable; and, in this case, an advantage will result from the inquiry, that we shall be assisted in distinguishing between the effects of remedies and the natural course of disease. It should be mentioned, that fever terminates on a certain fixed day, much more frequently at one time than at another, and the termination seems to be more irregular in winter than in summer."

The mortality of the epidemic, fortunately, did not keep pace with its extension. The deaths, during 16 months, from Jan. 1817 to April 1818, were, of 2883 males, 179, or 62 in 1000; and of 2849 females, 141 died, or 49 in 1000; the disease being more fatal in the former, in the proportion of 62 to 49. Very few children died. The disease, however, was observed to be much more dangerous to the upper ranks than to the lower. During two periods, of seven years each, from the opening of the hospital in 1804 to 1811, and again, from 1811 to 1818, the average mortality was, in the former period, one in 12; in the latter, only one in 18; but the *actual amount* of the mortality, during the latter period, was much the greater.

Respecting the appearances on dissection, Dr Barker has given a statement, (which we should have been glad to have inserted in full,) from Dr M'Cartney, professor of anatomy in the university of Dublin, who states the following, as the morbid appearances belonging strictly to typhus.

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* One of these tables is given in the present report at length; the plan is excellently calculated for establishments where diseases of one kind only are admitted.

> "1st, Fulness or distention of the vessels of the brain, especially the veins; some water effused on the surface and into the cavities of the brain. 2d, The same species of congestion in the lungs; and different degrees of effusion in the cavities of the pericardium and pleura. 3d, Venous congestion in the liver, spleen, or alimentary canal; sometimes a blood-shot appearance, or spots of extravasation in the mucous coat; more particularly in the stomach, and first coils of the intestines; in some instances, a more general pulpy or swollen and discoloured state of the mucous coat of the alimentary canal. These congestions were always of a purple or venous colour; and the blood, throughout the body, appeared to be accumulated in the venous system, and had little tendency to coagulate."

We regret that the observations of this distinguished anatomist were not extended to the medulla spinalis, which the statements of some recent observers, especially of Mr Sym,* prove to have been materially affected in certain modifications of fever; conceiving, as we do, the morbid conditions of that portion of the nervous system to hold an important place in the true pathology of the disease. Dr Barker, however, considers the coincidence between Dr M'Cartney and some of the best modern observers on this subject, as sufficiently proving, that the congestions remarked after typhus fever differ from those of genuine inflammation; and he is therefore of opinion, that the question of treatment, as founded on these appearances, remains pretty nearly as before,—to be determined by experience only.

The present report is very brief upon the treatment of fever, which did not indeed form any part of its immediate object. In some cases of danger, with apparent congestion in the brain, small doses of calomel, with antimonial powder, at short intervals of one or two hours, seem to have produced a favourable result. Wine was very little used. The purple colour of the extremities already noticed, appeared in a few cases to be diminished by detraction of blood from the head, whence it would seem that this symptom arose from, or was connected with, congestion in the brain.—When much local congestion was present, relief was obtained by general blood-letting, more especially if the symptoms indicated inflammation; "and, in such cases," Dr Barker adds, " the principle laid down by J. P. Frank, about twenty-eight years ago, was observed. ' In petechiis cum inflammatoria febre incidentibus, non posse modo, sed et debere, sanguinem pro impetus febrilis ratione educi, jam diximus." Upon this last point we suspect that we do not quite agree with the learned reporter ;—for although bleeding in continued fever was employed by Frank, as it had been by other practitioners long before him, we really consider the revival of that remedy as equivalent to a new discovery; and regard it as one of the few improvements in medical practice, the merit of which belongs to our own time.

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* See our Journal for July 1818, Vol. XIV. p. 336, &c.

The immediate CAUSE of the disease was unquestionably contagion ; aided, no doubt, in an extraordinary degree, by several concurrent circumstances. At first, however, the contagious nature of the malady was not apparent; and of ninety cases received at the Fever Hospital, twenty-four only could be traced to infection ; but as the epidemic advanced, its true nature became very evident ;* and from a table of eighty-nine cases, under Dr Barker's care, stating the number of persons previously ill in the dwellings from whence each patient had come, it appears, that, in twenty nine cases only infection could be traced ; in all the remaining instances some individuals, frequently several, had suffered in every house, within two months before. And proofs are given that this spreading of the disease was by no means confined to the houses of the lower classes. In some of the more wretched dwellings, notwithstanding the whitewashing and cleaning of the walls and floors, the fever still kept possession, attacking several successions of inmates, and was not finally overcome until the bedding and wearing apparel also had been thoroughly cleansed. One such mansion, which, during four months, was the receptacle of contagion, contained twenty-three inhabitants, every one of whom had the disease ; and those who succeeded them as tenants were equal sufferers.

Among the causes which co-operated with contagion, scarcity, with its companions, poverty, famine, and want of cleanliness, was the most important; the distress throughout the country having caused a vast influx into Dublin of strange poor, who, it will be recollected, are not restrained in Ireland from migrating, by poor laws. The connection between pestilence and scarcity had been frequently observed before in Ireland ; and in 1740-41, a year of scarcity was followed by an epidemic fever, of such severity that it was calculated that, during these years, 80,000 persons died of fever, dysentery, and famine. The distresses of the poor were greatly aggravated by cold; the turf or peat, which in Ireland is the chief fuel of the lower classes, being so scarce, that coals imported from the north of England were sold at twenty miles from Dublin at a cheaper rate, even with the addition of land-carriage, than turf. The baneful operation of these evils was aided by the habitual want of cleanliness of the lower Irish —want of fuel, causing the wretched sufferers to crowd together to small apartments, with clothing already saturated with offensive effluvia, and frequently worn day and night for long periods. Dr Barker mentions instances of eight or ten lodgers having been crowded into an apartment eight feet square in horizontal dimensions, and not more than eight feet in height.

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* Upon this subject Dr Barker refers to a recent publication by Dr Stokes of Dublin, entitled " Observations on Contagion." 2d edition. Dublin, Hodges and M'Arthur, 1818

It is, however, doubtful, whether the causes above mentioned, though powerful allies of contagion, would have been sufficient to its production. The true origin of the malady in its present aggravated form, is by Dr Barker ascribed to the introduction of new-contagion from the continent of Europe, where it had been generated among the armies employed there during the war. New contagion, he observes, is always the most active; and the increase of fever during late years coincides, as to time, with our increased intercourse with the continent, by means of our armies. During the expedition to Walcheren in autumn 1809, typhus prevailed at Flushing; and of the 26,000 persons who sickened there, many must have brought fever with them on their return to these countries. In 1810, the admissions at the Dublin, Cork, and Waterford Fever Hospitals, exceeded 2500, having been but 1500 the year before. And, again, in 1815, the increase of fever in Dublin followed a year of constant and extensive intercourse with the continent of Europe. Dr Barker has collected several other remarkable proofs, many of them taken from our volumes, not only of the connection between war and pestilence, but more especially, that, of late years, petechial fever has accompanied the progress of the great armies. Thus it made great havock among the French in the retreat from Russia. It was epidemic at Leipsic and Dresden in 1813; and at Altona, in the beginning of 1814; having spread over the greater part of Germany, from the place where the French entered that country on their retreat. About the same time the disease reached Paris; and was very fatal in Spain. and soon afterwards, the registry of the Dublin Hospitals shew a great increase in the number of fever cases. From 1815 to 1817, accounts from the Continent are wanting; but there is no reason to imagine that the progress of fever had abated; and, in the spring of 1817, it prevailed in many parts of Europe, at Turin in Savoy, at Rome and at Venice, where its introduction was ascribed to prisoners brought into the north of Italy. It is the same evil, Dr Barker asserts, which has more recently extended itself to Glasgow and Edinburgh, and has been very generally diffused in England.

The measures recommended to stop the progress of this formidable malady in Dublin, are principally the establishment of fever wards in different quarters of the city; and, in order to prevent the influx of infected persons into the metropolis, Dr Stokes has further suggested the formation of small hospitals in its vicinity. An excellent plan of co-operation with the hospitals is adopted in the parish of St Peter, Dublin, where an association of physicians has undertaken the duty of inspecting the houses of the poor, for the purpose of sending the sick to hospitals, and obviating the concurrent causes of the disease. In the same parish, also, an admirable system is employed for cleansing the clothes and wearing apparel, not of the sick only, but of the families from whence they have proceeded, and purifying their apartments and furniture. For these purposes a house was taken, and provided with necessary apparatus for washing, and with a stove for exposing to a high temperature such articles as could not be washed. The expences of this house from April, when it was opened at an outfit of L 37, to last October, have not exceeded L. 12 per month; and during that interval 1077 persons have had their clothes and bedding purified; and 3571 rooms have been whitewashed, and straw supplied to their inhabitants. Dr Barker calculates that eleven such houses would serve for the whole city; the outfit of which would not much exceed L. 400, nor the annual expence L, 1300. The establishment of public wash-houses, like that of Glasgow, is a measure obviously fraught with great advantages to large cities, especially where the supply of water and of fuel is not abundant, as appears to be the case in Dublin. Dr Barker justly considers the efficacy of fumigation as not yet established by well-conducted experiments. It may be useful, he thinks, under the management of intelligent persons; but ought not to supersede the most approved methods of thorough cleansing and ventilation.

We cannot conclude this article, without strongly recommending to the very intelligent author of the paper we have just examined, the republication, in a uniform series, of the several medical reports that have been issued by the Fever Hospital at Dublin. Such a collection, accompanied by a statement of the economy of the house, and a plan of its accounts, (which we know to be admirably conducted), would afford to physicians materials of great value upon the history of fever, and would be very useful to the managers of similar institutions;—and it would place, also, in a just light, the merit of those excellent persons, who, in conducting this establishment at a time when ordinary minds might have shrunk from the duties of that office, have never once relaxed in their superintendence.

VI.

The Medical Intelligencer.—No. I. Pp. 8. 8vo. Published by BURGESS & HILL, 55, Great Windmill Street, Hay-Market. 1818.

THIS is a work which we candidly confess we have a selfish motive in recommending very strongly to the patronage of our readers, as greatly facilitating the knowledge of recent medical literature, and the progress of the science in general. The first part contains a transcript of the contents of the medical periodical publications of the day. *viz.* in the number before us, the numbers published during October, November, and December, of the London Medical Repository; Medical and Physical Journal; Medico-Chirurgical Journal; Edinburgh Medical and Surgical Journal; and Quarterly Journal of Foreign Medicine and Surgery. Next, the titles of all the papers on medical and chemical subjects, reviews of medical books, in the Transactions of societies and journals, magazines, and reviews, not confined to these departments of science, *viz.* in the Philosophical Transactions of the Royal Society of London; Journal of Science and the Arts; The Annals of Philosophy, by T. Thomson, M. D.; Monthly Magazine; New Monthly Magazine; Edinburgh Review; Monthly Review; European Magazine; Literary Panorama; and Gentleman's Magazine. Lastly, the Intelligencer contains a more complete list, than any we have seen, of the New Works, New Editions, and Works in the Press, relating to our art; to which is added, a short list of foreign works, imported at reduced prices. It is also promised, "that at the end of every year, an Index of all the works published within that period, and of the various Papers contained in the Transactions of Societies, as well as those in the Quarterly and Monthly Journals, shall accompany the last sheet in the year, and that this list or index will be arranged alphabetically, according to the diseases treated of, with the authors' names following." We request the Editor to make this index very complete, both as an *Index Rerum et Auctorum.* Upon its perfection the success of the undertaking will greatly depend. The plan is good, and might be extended, with advantage, to the foreign Journals and Transactions of societies. Great attention should be paid to accuracy in printing proper names.

PART III.

MEDICAL INTELLIGENCE.

Extracts from a Letter from Thomas Young, M.D. Physician to St George's Hospital, London.

Account of a Case of Poisoning by Stramonium, which terminated favourably.

Having lately seen in the newspapers an account of the death of a child from eating an apple of the stramonium, I think it right to record a more successful case of a similar nature, which occurred to me the year before last.

A boy, between two and three years old, ate a whole apple of the stramonium, and it was nearly two hours before the fact was ascertained. A peculiar wildness of the look, with general suffusion, and some convulsion, having been observed, and his brother, not much older than himself, having had the sagacity, upon being questioned, to point out the plant, I immediately ordered a solution of the sulphate of copper, well known as one of the most prompt and active emetics; and about a grain and a half succeeded in operating with considerable energy, and bringing up a little more than a third part of the noxious substance. After this the same medicine was repeated, together with some antimonial wine; but the symptoms continued, and were rather increasing than diminishing. Not wishing to give larger doses of drastic and irritating medicines, I ordered a tea-spoonful of castor oil to be given every ten minutes; and this was done for three or four hours before the effect was produced, which was fortunately that of full vomiting, and the discharge of the greater half of the apple, together with the relief of all the alarming symptoms. A few hours afterwards, the remainder of the oil, which had been taken, produced its ordinary effect; and the next morning the boy was in his usual health.

It appears to me that, for all poisons which cannot be neutralized by some chemical antidotes, the administration of oil must be the grand and general remedy; castor oil, perhaps, if it is at hand, and if not, sallad oil, or any other oil, in very large quantities. Undoubtedly it is highly judicious to decompose corrosive sublimate by soluble alkalis, and to combine oxalic acid with magnesia; but in the case of arsenic, we have no right to expect any benefit from either acids or alkalis; and even dilution, by an aqueous fluid, might do harm by diffusing the mischief; but oil will sweep out the contents of the stomach, and at the same time sheath its coats from their noxious action, without extending its sphere, whether they may be of a mineral or vegetable origin.

I shall beg leave to take this opportunity of making one or two remarks of somewhat less importance to the public, though due to individuals concerned. In the extract, which you did me the honour to make from my book on consumption, (Number 52, page 500,) you have copied one of the very few mistakes which are corrected in the errata page, without noticing the correction, and have thus extended the injustice of which I had involuntarily been guilty to the memory of Mr John Hunter, in not attributing to him the discovery of the globules of pus. There is also a little suggestion of my own, which I must take the liberty of pointing out, in my Medical Literature, 1813, page 424. "I have often wished to try ligatures of catgut, which might be absorbed." I had, indeed, proposed this practice ten years ago to several surgical friends, entreating that they would make the experiment; and had mentioned it in the Medical and Chirurgical Society; and I am now truly happy to find that it has succeeded in the hands of Mr Astley Cooper. One of the persons, to whom I suggested it, had himself made a trial of *silk-gut*, without any decided success; and it is probable that this substance is equally indigestible, and incapable of absorption, with the ligatures commonly used; while catgut, which is nothing but membrane and muscle, must be very easily removed by the ordinary action of the absorbents.

Welbeck Street, 26th November 1818.

Extract of a Letter from Mr Hennen, Deputy Inspector of Army Hospitals, dated November 20th 1818.

Since my communication to you on the eruptive diseases observed in the military hospitals of this city, I find that an epidemic prevailed in the military hospitals of some parts of France upwards of half a century ago, in which the same individual was twice attacked with small-pox. A learned friend of mine, who pointed this out to me, has nearly doubled the catalogue I gave you of authors on this subject; he will, doubtless, communicate the results of his inquiries to the public. Numerous reports have been addressed to me on the subject, particularly one from the 54th regiment at Sunderland, of a soldier, who had had small-pox some years ago, at the same time with his sister; she is very much marked; the brother also is much marked on the face and thighs, and had been frequently exposed to the contagion of the disease, but in September last was again attacked, and had the disease in a confluent form. Mr Redmond, the surgeon of the 54th, states to me, that the medical gentlemen of Sunderland, who have seen the case, all agree as to its character, as well as to that of the original attack.

From a private source, I have just heard of a young female who had totally lost her sight from small-pox, having undergone a severe attack of the modified disease last year, which has left very distinct pits over her body.

I beg to correct an error in my communication to you in page 425 of your last volume, lines 8, 10, 11, where I have inadvertently used the word *pustule* for *vesicle* ; the context, however, would sufficiently show my meaning.

Extract from a Letter from a Constant Reader.

You must have found that cases of purulent ophthalmia of infants are not of unfrequent occurrence, and often productive of considerable trouble to those in attendance, not to mention the unpleasant consequences that sometime remain ; I therefore think it a duty incumbent on me to mention a remedy which I have found in three instances of considerable benefit; the application of a blister to the nape of the neck. In the first instance, it was applied after the other remedies, generally recommended, had been tried, but in vain ; and I was surprised to find the immediate beneficial results, so that I was determined to use it in future in a more early stage ; since which I have applied it in two instances, with a similar result, within the first week, and the cases were completely relieved in ten days. I was prompted in mentioning the remedy as considering it not to be in general practice, particularly in the early stage of the complaint. The blister should not be very small.

I would also add, in confirmation to what has been mentioned in a preceding Number by a correspondent, that I have long experienced the good effects of purgation, and the repeated applications of blisters to the calf of the leg, as the best mode of treating phlegmasia dolens.

Can any of your correspondents account for the fact that infants, under the age of five or six months, never receive the infection of true measles, the *rubeola cum catarrho?* I think I have met with one case where the patient was at the age of twenty-one weeks. An infant under this age might have an eruption similar to measles, but without cough, the *rubeola sine catarrho.*

I would also at this time send you the notes of a case of hydatids expelled from the womb, attached to a perfect placenta, but without funis ; the breasts were flaccid ; patient considered herself four months advanced ; but I have unfortunately lost my notes of it.

Extract of a Letter from Peter Cullen, Esq. Surgeon, Sheerness.

Having observed, in some of the late monthly, or periodical publications, cases mentioned, of not only troublesome, but even fatal effects of hæmorrhage, arising from the extraction of teeth, I beg leave to suggest a very simple mode, which I have adopted with success, on such emergencies; and which, I am inclined to think, will be found effectual in most instances of the sort.

Take a small, fine, vial cork, of a size adapted to the socket from whence the tooth has been extracted, and the hæmorrhage proceeds. Then, with a small dossil of lint wet in Aq. Stypt. and put on the smallest end of the cork, push the cork into the bleeding orifice, pressing it firmly in till it be, as it were, wedged in the socket, and keep it there, as long as may be necessary, desiring the patient to further press against it with the teeth of the opposite jaw, till the bleeding be stopped, which it is almost instantly. This acts as a tourniquet, and gives you time to use whatever other means you may deem requisite. But it is seldom that any thing else is required.

In February last Dr Watt of Glasgow published a Prospectus, which was again reprinted in May, and accompanied with a specimen of a work to be entitled "*Bibliotheca Britannica*, or a general index to the literature of Great Britain and Ireland, with such foreign works as have been translated into English, or printed in the British dominions. Including also, a very copious selection from the writings of the most celebrated authors of all ages and nations. *In two parts.* In the *first* the *authors* are arranged alphabetically, and of each, as far as possible, a short biographical notice is given, to which is subjoined a correct list of his works, their various editions, sizes, prices, &c. and in many instances the character of the works. In the *second* the *subjects* are arranged alphabetically, and under each all the works and parts of works treating of that subject, are arranged in chronological order. This part also includes the *anonymous works* which have appeared in this country, inserted according to their respective subjects and dates." Dr Watt having obtaned a pretty numerous and highly respectable list of subscribers, a first part of the work has been put to press, and will be published in February. This, consisting of 35 sheets, is estimated to be about a sixth part of the whole. A part will be published every three or four months till the whole is completed, making two handsome quarto volumes, of from 800 to 900 pages each. This publication is said to contain above 40,000 authors, and the titles of about as many anonymous works.

Speedily will be Published,

A Cornelii Celsi De Medicina Libri VIII. Cum notis variorum, aliisque obscuriora elucidantibus. Cui accedit Lexicon Anglo-Latino-Medicum, Latinè scribentibus utilissimum. By EDWARD MILLIGAN, M. D. Lecturer on Institutions of Medicine.

In the press, and speedily will be published, by Bell and Bradfute, Parliament Square, Edinburgh, in one volume octavo, A Practical Treatise on the Efficacy of Blood-letting in the present Epidemic Fever of Edinburgh, Illustrated by numerous Cases and Tables from the Journals of the Queensberry House Fever Hospital. By **Benjamin Welsh**, M. D. Senior Clerk and Superintendent of that Institution.

Mr **Coar**'s edition of the Aphorisms of Hippocrates, in Greek, Latin, and English, is now in a state of forwardness, and will be published within a few weeks. Gentlemen desirous of adding their names to the list of Subscribers, are requested to send them to the Publishers, Cox and Son, St Thomas's, Borough.

A course of Lectures on the congenital malformations and morbid and accidental Distortions of the Bones will be delivered in January (1819) by G. H. **Weatherhead**, Graduate of the University of Edinburgh, Surgeon of the Royal College, Edinburgh, Member of the Medical and Chirurgical Society of London, &c. at his house, No. 18, Upper Montagu Street, Montagu Square.

December 1818.

No. LIX. will be Published on the 1*st April* 1819.

THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL.

No. LIX.

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ERRATA.

Page 42, line 15 and 16, *for* modified horn-pox *read* modified small-pox
46, line 20, *for* Old Kirkpatrick *read* Old Kilpatrick
47, line 11, *for* dropsical *read* tropical
49, last line, *for* laeta victoria *read* victoria laeta
50, line 20. *for* Mr Johnstone *read* Mr Johnson

In Mr Brown of Musselburgh's paper, the foot note in page 79 should be in page 78, with the asterisk at the word surgeon, line 28th of that page.

We have received communications from Dr J. Thomson, Thomas Barnes, J. Forbes, W. Crane, and M'Sweeny, and from Messrs Benj. Granger, E. Turner, Wood, Gourlay, and James M'Intyre.

Reviews have been prepared of Brodie on Diseased Joints; Allan's System of Surgery; Ayre on Marasmus; Majendie on Gravel; and the Med. Chir. Trans. Vol. IX.

We have received for review,

Memoir on the Formation and Connections of the Crural Arch and other parts concerned in Inguinal and Femoral Hernia. By Robert Liston, &c. 4to. Edinburgh, 1819. 23 pages and three plates.

A Treatise on two of the most important Diseases which attack the Horse. Part I. Locked Jaw—Part II. Epidemical Disease, or Catarrhal Affections. 4to. By Will. Wilkinson. Newcastle. 1818. 212 pages, and one plate.

Elements of Medical Logic, illustrated by Practical Proofs and Examples, including a Statement of the Evidence respecting the Contagious Nature of the Yellow Fever. By Sir Gilbert Blane, Bart. 8vo. London, 1819. pp. 219.

An Essay on Fever, to which is annexed a Critical Review of the letters which have lately appeared in the Newry Telegraph, relative to the comparative respectability of Physicians, Surgeons and Apothecaries; by John D. Ferguson. 12mo. Newry, 1818. pp. 23, 48, 55.

On the Mimoses; or a descriptive, diagnostic, and practical Essay on the Affections usually denominated Dyspetic, Hypochondriac, Bilious, Nervous, Chloritic, Hysteric, Spasmodic, &c. By Marshall Hall, M.D. 8vo. London, 1818. pp. 176.

Elements of Modern Chemistry. By M. P. Orfila; translated by Sir John Byerly, Bart. Volume I. 8vo, London, 1819. pp. 310, with seven plates.

Essay on the Shaking Palsy. By James Parkinson. 8vo. London, 1817. pp. 66.

Observations on the Necessity of Parochial Fever Wards, with Remarks on the present extensive spread of Fever. By James Parkinson, 8vo. London, 1818. pp. 20.

Pharmacopœia Castrensis Ruthenica. Auctore Jacobo Wylie, Eq. Baronett. Editio auctior. 8vo. Petropoli, 1818. pag. xvi.—316—xxxii.

An Essay on the Diseases of the excreting parts of the Lachrymal Organs. By William Mackenzie. 8vo. London, 1819. pp. 95.

Medical Sketches: 1. On the Use of Hellebore, as a Remedy for Insanity. 2. Of Colchicum autumnale, and its use in Medicine. 3. Observations on the Sudden Death of Women in Childbed. By George Kerr. 8vo. London. 1818.

An Appendix to the Pamphlet on the early Symptoms of Water in the Brain; containing Cases successfully treated; with Practical Illustrations of the Doctrines therein inculcated, and some Observations on the Functions of the Intestines, as connected with a morbid action of the Digestive Organs. By G. D. Yeats, M. D. 8vo. London, 1819. pp. 97.

Bennet's Observations on the Doctrines of Life.

Dickinson on Yellow Fever.

Power's Treatise on Midwifery.

Mills's second edition of the Morbid Anatomy of the Brain.

Mill's Inquiry into the Effects of Diseases of the Liver, or other Viscera.

THE

EDINBURGH
MEDICAL AND SURGICAL JOURNAL.

1. APRIL 1819.

PART I.

ORIGINAL COMMUNICATIONS.

I.

Observations on the Management of Tedious Labour, with an Account of the Number of Cases that occurred in the Lying-in-Hospital, Dublin, from November 1800 *to the* 31*st July* 1816.*

By John Breen, M. D. late Assistant to that Institution, and Licentiate of the King and Queen's College of Physicians.

The principal part of the following paper was published in the year 1808, in the Dublin Medical and Physical Essays. The facts, adduced in support of the conclusions, occurred in the Lying-in-Hospital of this city. The objects of the publication were, first, to approximate towards ascertaining the greatest length of time, for which we might confide in the efforts of nature, in a tedious labour, when the head presented; and secondly, to prove that it might be laid down as a general practical rule, that, during such a labour, for a period not exceeding thirty hours, unless accidental circumstances occur, the patient will go on with safety.

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* The writer terminated the period for which he was elected assistant in August 1806.

By accidental circumstances are understood rupture of the uterus, puerperal convulsions, disease prior to the commencement of labour, preternatural presentation, and hæmorrhage. It will be demonstrated, by a deduction from 11,695 cases, that the occurrence of any of the casualties in this formidable list is so rare as not to affect the general reasoning.

At the period of first publication, it was the intention of the writer to have made that essay the first of a series on general subjects of midwifery; therefore, a number of facts not very strictly connected with the title of the paper were brought together. The discontinuance of the Dublin Essays shortly after this publication, and the pressure of a harassing profession, have hitherto delayed the fulfilment of that intention.

A republication, though in a somewhat different form, seems warranted by the reference of Mr Burns to the tables, in his Principles of Midwifery, page 242, in which, however, he has not sufficiently adverted to the end for which they were constructed; and by the repetition of Mr Burns's reference in Dr Merryman's Synopsis.

The terms tedious labour, when used in this paper, signify labour where the head of the child presents, and a period exceeding thirty hours elapses, from the commencement to the termination. Every practitioner of midwifery must acknowledge the importance of the treatment of this species of parturition. In the various other embarrassing circumstances that occur to the accoucheur, the principles which should regulate his conduct are more fixed, and nearly demonstrable; and numerous well authenticated cases are on record to guide his judgment. When the patient's sufferings are so long protracted, she and her friends become uneasy and anxious. The practitioner, unless he has reason to expect a speedy termination of the case by the efforts of nature, will begin to feel apprehensions lest he should be under the necessity of having recourse to instruments. A moral question also, of very serious importance, sometimes comes under consultation, as cases occur where the means best calculated to relieve the parent are incompatible with the safety of the child. In choosing the time of affording extraordinary assistance, it should be kept in constant recollection, that even when some disproportion exists between the pelvis of the mother and head of the child, the long continued action of the uterus gradually compressing and elongating that part, will expel a living infant where either the forceps or lever would do so much violence as to injure the parent and destroy the child.

That the reader may have an opportunity of estimating the fidelity of the statements contained in this paper, it becomes necessary to give an account of the Lying-in-Hospital of Dublin. It owes its institution to the exertions of the late Bartholomew Mosse, M. D. and was opened in December 1757 for the reception of patients. From that period to the 31st of December 1807, there have been delivered 59,354 women of 31,559 boys, 28,882 girls, 1030 have had twins, 19 had triplets, and one had four children at a birth, 621 women died, and 3266 children were still-born.* By the benevolent regulations of the institution, the gates are at all hours open for the admission of patients, who are not under the necessity of procuring recommendations. The governors are incorporated by charter, and limited in number to sixty. The general management of the patients is more particularly committed to the Master, who has two Assistants chosen by the governors, on his recommendation, joined with him in the care of the hospital. There are also six male pupils generally residing in the house, each of whom continues for six months. The pupils have the freest access to the wards, and to the hospital books. A considerable proportion of these pupils commence their attendance at the Lying-in-Hospital, after a regular medical education, and having either taken the degree of Doctor of Medicine, or having received letters testimonial from the College of Surgeons in Ireland. The Master, by the enactments of the charter, cannot hold his situation for a longer time than seven years, and having served that period, cannot, on any future occasion, be re-elected. By similar clauses the assistants hold their situations but for three years. In the election of master, the governors are bound to give a preference to those who have been assistants. The late Dr Kelly was master of the hospital during the interval, more particularly noted in this paper. The assistants, in the order of seniority, were Dr Ousely, Dr Labatt the present master, Dr Fergusson, and the writer of this paper, together with Mr A. Johnston, now Professor of Midwifery to the College of Surgeons in Ireland. The hospital books, from which the statements of this essay are extracted, were kept by these gentlemen. Two of the number, Drs Labatt and Fergusson, were joint editors of the Dublin Essays, in which publication the writer first communicated his opinions.

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* The proportion of children brought forth by such a number of women appears to be greater in Ireland than in any other country, from which we have published accounts of the number of females having plurality of children. On this subject, see a paper of Dr Gartshore, in 77th Vol. Transactions of the Royal Society of London. Also one by Dr Bland in the 71st Vol. The proportion in the hospital, Paris, as appears by Madame Boivins, was 1 in 129, in ours 1 in 56.

Between the commencement of November 1800, and the end of July 1806, 11,695 women were delivered, of which number 148 died from the following causes:

Fever, including Puerperal and Typhus, . .	46
In consequence of tedious labour, without use of instruments,	23
After tedious labour, where instruments were used, -	18
Miscellaneous causes, which include Mania, Dysentery, organic affection of the heart, Pneumonia, &c. - -	14
Spontaneous rupture of uterus, - -	12
Hæmorrhage, - - - -	9
Phthisis Pulmonalis, - - -	7
Puerperal Convulsions, - - - -	6
After hand and shoulder presentation, - -	6
Twins, - - -	5
Triplets, - - . - -	2
Total Deaths,	148

It is necessary to observe, that no general inference as to the mortality of women dying in labour can be drawn from this statement, as by necessary consequence of the system of the institution, women have been received in every disease to which females, during pregnancy, are liable, and even on the verge of dissolution, from previous mismanagement and illness. In 1803 likewise, the puerperal fever was epidemic in the hospital. In 1804, the number of beds was increased, and has been since further increased, which will certainly tend to diminish the frequency both of puerperal and typhus fever.

Tedious labour, terminated by the efforts of nature without instrumental or manual assistance, took place 290 times in the 11,695 cases. A few cases of plurality of children took place, that might be referred to this head; but, as it was the more common practice of the hospital in twin cases, to leave the expulsion of the second child to nature, after rupturing the membranes, where either the head or breech presented, I do not class such occurrences with tedious labour. Of the 290 there were 196 cases of first children, and 137 living children were brought forth. The remaining 94 had born a child or children before, and 67 living children were produced by these 94 patients. The deaths in the 290 cases were 23, the number of living children born by these 23 was only seven. I have constructed the following tables, in order to give an accurate and comprehensive view of these facts.

The Table, No. I. shews the length of time the patients who brought forth a first child in tedious labour were ill. The periods of time are distributed in intervals of ten hours, beginning from between 30 and 40 hours, and marking every intermediate space of 10 hours to between 110 and 120 hours. Under each period of time the number of women who were delivered is stated; the number of living children born by these women; and lastly the deaths.

Table—No. I.

First Children.

Between	30 & 40	40 & 50	50 & 60	60 & 70	70 & 80	80 & 90	90 & 100	100 & 110	110 & 120
Women	34	102	11	8	24	4	12	0	1
Living Children	27	72	8	5	15	2	7	0	1
Women Dying	1	8	1	1	2	0	2	0	0

Thus, in the preceding table under the figures 30 and 40 in first division, which express between 30 and 40 hours' labour, are the numbers 34, which shew that there were 34 women between 30 and 40 hours in labour of their first child, and that of this number 27 had living children, which is stated in the third division, and the casualties of the women are expressed in the fourth division, which exhibits one death among the 34. Every succeeding column is to be understood in the same manner.

Fifteen deaths in 196 cases of tedious labour of first children are stated in Table, No. I. of which an account is now to be given.

The death in the first division proceeded from typhus fever.

Two of the eight deaths in the second division appear to have been chiefly caused by ill treatment before admission, one ill treated by her husband, the other by a midwife. One died of typhus on the 19th day after delivery; one of puerperal fever; one of diarrhœa and difficulty of breathing on the ninth day after delivery. One is stated in the book to have died of debility, but no marked disease. The cause of the death of one is not accurately recorded; but I have reason to believe it proceeded from debility. The remaining death, not yet accounted for, arose from phrenitis.

The death in the third division arose from debility 30 hours after delivery.

The death in the fourth division, fever on the 14th day after delivery.

Both the patients whose deaths are marked in the seventh division died of fever, one on the 10th day. The day of the death of the other, who was delivered of a dead girl after being 100 hours in labour, is not accurately noted.

Table—No. II.

Between	30 & 40	40 & 50	50 & 60	60 & 70	70 & 80	80 & 90	90 & 100	100 & 110	110 & 120
Women	23	48	6	1	9	0	2	0	1
Living Children	19	36	4	0	7	0	0	0	1
Women Dying	2	5	0	0	0	0	1	0	0

It now remains to give a concise account of the eight deaths in Table II.

In the first division one died of typhus fever on the eighth day after delivery; and, from recollection and the best account I can obtain, the other laboured under disease on admission.

Three among the five deaths in division the second arose from puerperal fever. One from debility thirty hours after delivery; and the other was under the care of a midwife, and ill treated before admission.

For the remaining death, the patient was ninety-six hours ill, and died of fever on the third day after delivery.

By the preceding account of deaths it will be seen, that there were eight cases of fatality from typhus fever. These may be considered, in a great measure, to have died independent of tedious labour, which, I conceive, acted merely as a predisposing cause to the disease. At the same time it is obvious that the chance of recovery in an attack of typhus fever is much greater after an easy labour than after a difficult. In private practice a similar occurrence is not likely to take place.

In the 11,695 cases, exclusive of convulsions, hæmorrhage, and ruptured uterus, instruments were used forty-four times. The following tables will, at a view, shew the length of time the women were ill, and also exhibit the mortality after their use.

The Table, No. III., shews the number of hours the women who were delivered with instruments, and who recovered after their use, were in labour, the times divided into partitions of five hours.

Table—No. III.

Less than 30 hours.	Between	30 & 35	35 & 40	40 & 45	45 & 50	50 & 55	55 & 60	60 & 65	65 & 70	70 & 75	75 & 80	80 & 85	85 & 90	90 & 95	95 & 100	100 & 105	105 & 110	110 & 115	115 & 120
1 Woman	Women	1	1	0	9	3	0	0	0	5	1	1	1	0	2	0	1	0	0

The Table, No. IV., shews the same facts with respect to the women who died after the use of instruments, that the Table, No. III., shewed with respect to those who recovered.

Table—No. IV.

Less than 30 hours.	Between	30 & 35	35 & 40	40 & 45	45 & 50	50 & 55	55 & 60	60 & 65	65 & 70	70 & 75	75 & 80	80 & 85	85 & 90	90 & 95	95 & 100	100 & 105	105 & 110
3 Women	Women	0	0	0	6	0	1	0	1	3	3	0	0	0	0	0	1

Of the eighteen who died four were sent to the hospital after being long under the care of a midwife, their strength much exhausted, and dissolution took place shortly after.

Two were delivered by instruments in consequence of acute inflammation occurring during labour; in one the cause of death was ascertained by dissection, and the inflammation seemed to have commenced in one of the ovaria. The other received an injury by a fall shortly before admission, and symptoms of abdominal inflammation were violent before delivery.

Mary Clarke, delivered in consequence of umbilical hernia, died shortly after delivery, was not more than twenty-four hours in labour.

Isabella Legget, somewhat deformed, delivered by instruments after being one hundred and ten hours in labour; was out of bed for some days, and considered free from danger, but was attacked on the seventeenth day after delivery by violent symptoms of internal inflammation, which carried her off in twenty-four hours.

The remaining ten appear to have died either from the effects of debility or inflammation.

The proportion of deaths here stated will to many probably appear large; and whether the use of instruments, in the cases of some of those who died after tedious labour, as well as their earlier use in the cases of those who died where they were ultimately applied, would be better practice, is not for me to determine; but as I have laid the facts before the public, nearly without comment or defence, it is submitted to discussion. I will, however, observe, that in an hospital, particularly when it is unhealthy, the chance of recovery after either tedious labour or instrumental delivery, will be less than under favourable circumstances in private practice. At the same time, I have no hesitation in avowing, that, in some of the cases where I was concerned, it was my wish after the termination of the case, and I believe also that of the other gentlemen with whom I was joined in the care of the hospital, that we had earlier delivered our patient. Such was my recorded opinion ten years since; and I am fully satisfied that we rather erred on the side of not using instruments sufficiently often, than on the side of using them too frequently. A comparison of our instrumental cases with those of the Hospital de la Maternité, Paris, as given by Madame Boivin in her Memorial de l'art des Accouchemens, Paris, 1817, and of Drs Bland's and Merryman's cases, as published by the latter in his Synopsis, London, 1814, will enable the reader farther to appreciate the accuracy of this opinion. In addition to the forty-four instrumental cases already stated, we had eighteen, which gives a total of sixty-two in 11,695, or one in one hundred and eighty-eight. In the hospital at Paris there were one hundred and twelve in 20,517, or one in one hundred and eighty-three. Dr Bland's cases were as one in one hundred and fifty-eight. Dr Merryman's as one in ninety-four.

It is unnecessary to enter with any degree of minuteness into the general plan of treatment of women in labour, which is well detailed in the different elementary books of midwifery in the hands of every practitioner. The propriety of opening the bowels is so universally known and assented to, that I should not allude to it, but for the purpose of strongly recommending, either in tedious labour, or when the labour is expected to be so, that purgative medicines should be trusted to rather than enemata. The ordinary house pill of the Dublin Lying-in-Hospital consisted of equal parts of scammony, aloes, and soap, made into a mass with jelly of soap. This form of pill was frequently administered, as was also sulphate of soda and castor oil.

I should next wish to urge, what indeed can only be necessary to the younger practitioner, that the accoucheur should with firmness resist unnecessarily attending on the patient when there is no prospect of her being speedily well. By yielding to this unnecessary attendance the practitioner becomes exhausted, perhaps impatient, and disposed to hasten parturition when it ought not to be done, and from fatigue runs a risk of not being in possession of that coolness and presence of mind which may be so necessary for the benefit of the person committed to his care. A firm and temperate explanation of the causes of our conduct will generally satisfy the patient and her friends.

I have next to say a few words on the use of opiates. Whatever opportunity of observation I have had induces me fully to concur in the opinion of Dr Denman on the administration of opiates, * and against their early use. Good effects appear to me frequently to have followed the administration of from twenty five to thirty drops of laudanum, given in the course of the second night's labour. Where sleep was not produced it often removed the anxiety and uneasiness which occasionally remain in the intervals of pain. Every person accustomed to observe the progress of tedious labour must have taken notice of a tendency in the action of the uterus to be partly suspended and again renewed. I would almost go the length of saying, that something like critical hours may be observed. By endeavouring to time the administration of an opiate with the tendency of the uterus to cease its action, I think I have observed that degree of refreshment procured, that in a labour, where it was apprehended the patient should be delivered with instruments, uterine action again commenced with vigour, in the course of a few hours, and the child has been expelled by the efforts of nature. Some practitioners are partial to the administration of opiates in small doses at short intervals. I have seen this method repeatedly tried, and observed its effects with attention, and I think without prejudice ; but I cannot say that any good effects have followed from this mode of administration. I must add, that I have known laudanum to be administered in a full dose, under what I conceived favourable circumstances, without any good effects ; but as it should be an universal rule never to administer opium in labour, unless the bowels be previously opened, as this precaution was always used in the cases here alluded to, I do not think it did injury.

Blood-letting, which many judicious practitioners recommend in tedious labour, was very rarely, if at all, tried in the Lying-in-Hospital, Dublin, for the purpose of expediting the tendency to dilatation of the parts concerned in parturition, or of obviating what may be denominated an excessive febrile tendency. The experience of twelve years has made me regret that I did not urge the more free and frequent use of the lancet in our

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* Introduction to Practice of Midwifery, Vol. II. p. 12.

hospital; and I now feel firmly convinced, that both theory and experience equally sanction the safety and advantage of venesection in tedious labour. I have heard the propriety of the practice ably inculcated, and strongly recommended, in the lectures of the present Professor of Midwifery in the University of Edinburgh.

I shall next speak of the attention proper to be given to the urinary secretion and discharge. The attendants are very apt to deceive us in their report of this latter circumstance; it is therefore the duty of the practitioner to ascertain it with precision, as I conceive it constitutes a very important circumstance in the treatment of tedious labour. I have known the catheter introduced three times in the course of a tedious labour, and no inconvenience follow from so long a retention. But I would wish to direct attention to a very diminished secretion, where only a small quantity of high coloured urine is found in the bladder on the introduction of the catheter. I do not recollect this circumstance being noticed before as taking place in tedious labour, at least I cannot call to my recollection having met with the observation in any writer on midwifery. I may here introduce the general observation, that as no memory can be so tenacious as to retain all the facts which are met with in the course of reading, whenever any fact, whose importance was before overlooked, is for the first time forcibly impressed on the mind, we are apt to draw the conclusion, that we then first notice what had escaped the observation of others. Whenever this symptom occurs, it seems to shew the existence of such a derangement in the system, as to warrant the expediting of the delivery.

This appears to be a proper place to mention, that I have met with two cases, in which I was called in, where the women were long under the care of a midwife, and in both, the bladder was amazingly distended with urine, but the urethra was so completely filled with mucus as effectually to stop the perforations of the catheter, and prevent any passing. I would conceive this circumstance sufficient to warrant an immediate delivery. In the cases here mentioned, one recovered, the other died. Since this was first published, a medical friend has suggested to me, that if I had carefully covered the perforations of the catheter with butter, this simple contrivance would have excluded the mucus, and the heat of the water in the bladder would have melted it, and allowed the urine to pass. * There

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* A canulated catheter, invented by Mr Samuel Young, and described by

are cases in Smellie, where he appears not so anxious about drawing off the water as I would think it prudent to be; but, in his cases, it will be observed, that the secretion went on regularly, which I conceive to be a matter of the greatest importance.

A violent rigor coming on after labour has lasted 29 or 30 hours, I conceive to be a sign that inflammation is about to set in; if no other unfavourable symptom be present, I would not think it to be a sufficient cause for instantly ceasing to trust to nature, but on the occurrence of this symptom we should watch the slightest changes of our patient with the greatest attention. The alterations of countenance, I conceive, furnish a very important criterion in the treatment of tedious labour. It is difficult, if not impossible, to give any rules on this head, more than to recommend an attentive observation of the slightest variation, or tendency to the sinking of countenance. Violent and acute inflammation, characterized by severe and unceasing pain, with vomiting, occasionally occur. There are two cases noted as occurring amongst the 11,695; in each it took place within twenty-four hours from the commencement of labour; in both, labour was so far advanced as to allow immediate delivery by instruments; each case terminated unfavourably, and the cause of death in one was ascertained by dissection.

To support the courage and confidence of the patient, constitutes a highly useful and important part of the duty of an accoucheur, in his attendance on a tedious labour. I would conclude, that, where the secretions go on regularly, light food is taken in sufficient quantity to support the strength, and remains on the stomach; the pulse and countenance continue good; vomiting is not violent; the vagina is well lubricated with mucus; and uterine action causes the slightest advance of the child. It is astonishing for what a length of time we may trust to nature, and what difficulties the long continued action of the uterus will surmount. The parts being protected by mucus, is a matter of considerable importance, as this secretion prevents local injury and inflammation. No artificial application appears to me of much consequence in supplying this defect of secretion. Emollient injections thrown up the rectum, tend both to promote the secretion here mentioned, and to increase uterine action. When some disproportion exists between the pelvis and child's head, together with this tendency to local inflammation, manifested by

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him in the Medical and Physical Journal for December 1805, Vol. XIV. appears well calculated to obviate this inconvenience.

the dry and tumid feel of the parts, it is a very embarrassing and distressing occurrence to the practitioner, as, unless the child's head be lessened, a risk is run of the urethra and neck of the bladder being injured, and eneuresis for life being produced. I have hitherto rather considered this description of labour as an uninterrupted series of uterine action, which, in the more tedious cases, was by no means the fact, but, on the contrary, an almost total cessation sometimes took place. Under such circumstances, when the strength does not appear much exhausted, an opiate may be given, and after its effect, if necessary, a slightly stimulating injection generally will excite uterine action.

Treatment after Delivery.

On this head it will be only necessary to say a very few words, as I conceive the treatment of diseases which occur after parturition, to be unconnected with my present subject. Attention to the state of the bowels, and the administration of gentle saline laxatives, we considered as our sheet anchor, if I may be allowed the use of this expression. This has been an established practice of the hospital for a great number of years. Shortly after allowing the patient time to recover from the first fatigue, if the bowels had been bound before delivery, or if opiates had been administered in labour, or immediately after, a gentle saline purgative was given. Fomentations of the abdomen, and local fomentations of the parts on the slightest appearance of a tendency to inflammation, were always had recourse to. When I now meet such an occurrence in private practice, on the inflammatory tendency being manifest, I direct blood to be taken from the arm, and immediately after bleeding, order a draught, consisting of six drachms of castor oil and two of rectified oil of turpentine. This mixture is rarely rejected by vomiting, even by such as have a great repugnance to castor oil. It, in most instances, quickly opens the bowels, and I know no other medicine that possesses so much efficacy in relieving flatulent distention, which is generally met with in such cases. As turpentine certainly communicates an odour to the contents of the bladder and rectum, it may not be considered an unreasonable hypothesis to suppose it to have some influence on the lochial discharge. In large doses I have not known it produce dysuria. * An opiate was not often given after either tedious labour or instrumental delivery; indeed, in the generality of cases, it was found, that the patient, after being properly settled in bed, was disposed to sleep, from the fatigue previously undergone. In cases of fetid lochial discharge, injections thrown up the vagina appeared to me to have done much service. A decoction of chamomile seems to answer very well for this purpose. Attention to the state of the bladder constitutes also an important part of the after treatment. It was very rarely found necessary to use the catheter after delivery. If within the first twelve hours water was not passed, the steam of boiling water was applied from a convenient vessel, which generally produced the effect wished for; and was also an application calculated to prevent local inflammation. But two cases came to my knowledge of injury arising to the urethra or neck of the bladder, either from the effects of tedious labour or instrumental delivery. One was a soldier's wife, who was delivered by the efforts of nature, after forty-eight hours labour; she was discharged from the hospital in the usual time, and was attacked with local inflammation, according to the most accurate account I could collect from her, in consequence, from her situation at the barrack, of being unable to pay the requisite attention to cleanliness.

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* Turpentine was first recommended in puerperal fever by Dr Brennan of this city.

It now remains to prove the assertion which was made at the commencement of this paper, that, generally speaking, labour may be considered safe to the period of thirty hours. This, I think, will be sufficiently manifested by a statement of the number of women who died that were above twenty hours ill, and who lost their lives from the different other causes that occasioned death than tedious labour, and after the use of instruments. There were only twenty-five patients that died after being twenty hours in labour, independent of the twenty-three deaths after tedious laborious labour, and the eighteen after instrumental delivery, of whom an account is before given.

The following circumstances caused these patients' deaths:

Ruptured Uterus, - - - -	5
Shoulder presentations, all of whom had been under treatment before they were sent to the hospital, -	5
Puerperal convulsions, - - -	4
Plurality of children, - - -	3
Phthisis, - - - - -	2
Lues, - - - - -	1

The preceding cases may be considered as excepted in the assertion under consideration. There therefore, only remain five to be accounted for, three of whom died of typhus fever, and were between twenty and twenty-five hours in labour. These three, with two mentioned as having been early delivered by instruments, in consequence of the acute inflammation having set in, constitute so small a proportion to the entire number of 11,695 cases, of which account is given, as not to affect the general truth of the proposition. The total number of cases that occurred, of women between twenty and thirty hours ill in the 11,695, was 484.

Of the causes of tedious labour, I have avoided making mention, as, in the first place, conceiving them closely connected with the physical education of the female sex, the state of society, and the management of women during pregnancy;—and, in the next, involving the subject of bringing on premature birth, and the mechanical assistance sometimes necessary to be given in labour.

Cavendish Row, Dublin, December 3, 1818.

II.

Case of Fungus Hæmatodes of the Left Eye, in which the Disease was removed by Extirpation. By ROBERT RENTON, Esq. Surgeon, Penicuik.

THE pathology of the disease which forms the subject of the present communication, until Mr Wardrop published his observations on Fungus Hæmatodes, was involved in considerable obscurity. There seems every reason to believe now, from the description he has given of the morbid changes produced in the different stages of the disease, that if any attempt should be made for its removal, it is only by the extirpation of the tumour in its primary state, and before the circumjacent parts have been contaminated, that we can expect to arrest its progress, or to effect a permanent cure. The diseased eye, which I had occasion to extirpate, was similar in every respect to that species of tumour he has described under the name of fungus hæmatodes; and particularly to that state of it before the tumour has burst, or the fungus has begun to discharge blood, and throw itself off by sloughing. I am sorry I had not an opportunity of observing the changes produced upon the structure of the eye from the commencement of the disease, the patient having long laboured under it before she applied to me. I have, therefore, only her own verbal account to give of the various symptoms with which she was from time to time affected during its progress, until the period she put herself under my management.

The disease, in the present case, was situated in the left eye, and, like most of those described by Mr Wardrop, appears to have been occasioned by a blow. This accident the patient mentions she received, so far as she remembers, about thirteen or fourteen years ago, in consequence of her striking her head against the latch of a door. The injury was upon the external angle of the left eye, and occasioned her considerable pain. This symptom, however, did not continue long, and a slight degree of ecchymosis, which was likwise an effect of the injury, disappeared in a short time, without any application being found necessary to remove them. About a year after this, the pain, without any apparent cause, attacked the part where the injury was received, and inflammation of the eye supervened, and both were removed by warm poultices, which she was recommended to use. The sense of vision was much impaired by this second attack, and she does not remember of having ever afterwards enjoyed so distinctly, as she was wont to do, the use of the left eye. About three months subsequent to this, these symptoms returned in an aggravated degree, together with a violent pain, which occupied the left eyeball, and extended from that part to the occiput, along the left side of the head. This attack she imputed to a slight injury she received upon the weak eye. It continued nine weeks, and was removed by the application of leeches. For five years subsequent to this period, she was not affected with any violent attack of pain, or of inflammation of the eye, and continued in good health, undisturbed with any other symptom, excepting the occasional return of headachs, which sometimes lasted two and three weeks at once. She had almost lost the use of sight, by the end of this period, her vision having become gradually more and more imperfect, so that she could now with difficulty distinguish day from night with that eye. About this time she was affected with a most severe weed, (rigor,) and mentions, as a consequence of it, that in the course of twenty-four hours, her eye became enlarged, and continued so for the space of ten or eleven weeks. By persevering, however, in a course of cooling salts, and by the frequent discharge of blood by means of leeches from the parts contiguous to the eye, that organ regained its natural size. During the next seven successive years, the inflammation, and sudden enlargement of the body of the eye, never returned to so great an extent, although her health was much impaired, and she was much distressed with headachs, which she attributed to the disease of her eye, and with frequent ephemeral attacks. In the autumn of 1817, the body of the eye became again enlarged, and continued gradually to increase in size, and the pain in that organ also became much more violent.

In the course of the summer 1818, I first saw her, and upon examining the left eye, observed the body of it greatly enlarged, and altered in shape and appearance. The conjunctive coat had entirely disappeared, and in place of the white colour of that tunic, the anterior part of the eyeball was covered with a tissue of blood-vessels, through which there appeared a membrane of a greenish taint. The tumour was round, smooth, prominent, and elastic to the touch, and of a shining appearance, as if its surface had been spread over with albumen. When measured across it is nearly three inches in breadth, and in the direction of the upper to the under eyelid, not above an inch and a half. The matter it discharged, which was not very considerable in quantity, was thin, and of a livid and purple hue. The under eyelid was everted; and in consequence of the weight and pressure of the tumour, there was considerable œdema of the subjacent parts. There was almost no trace to be discovered of the pupil. Half of the cornea had been destroyed, and its remaining half extended along the part the pupil should occupy, in the form of a white semicircular ridge. On the external angle of the eye, and upon the part where the injury had been received, great pain was occasioned by pressure. The tumour itself was not so sensible to pain upon being touched. She complained of lancinating pain in the eyeball, darting from that organ to the occiput, which she cannot touch without producing excessive pain. Her health has declined much of late; her sleep has been disturbed; and her appetite greatly impaired in consequence of this last attack. She is a married woman. Her name is Grizel Fraser. She is about forty years of age, and has particularly remarked, that, during the state of pregnancy, she had not been subject to the ephemeral attacks, or to the inflammation of the eye.

After attentively considering her case, the opinion I formed of its nature led me to recommend extirpation as the only possible means by which she could get rid of the disease. Indeed, she had suffered so much from lancinating pain in the orbit and head, and had become, in consequence of the duration of the last attack, so hopeless of receiving benefit from internal remedies or external applications, that I found little difficulty in persuading her to submit to an operation. Before, however, proceeding to this last expedient, I wished that her friends should be satisfied as to the propriety of it. For, independent of the unpromising result which the hitherto unsuccessful attempts to extirpate the disease held out against the operation, there were likewise other dangers to be incurred by the performance of it which it would be prudent to avoid. I therefore recommended her to consult another surgeon, and there was none to whose advice I could recommend her with more confidence than to my friend Dr Thomson. My son, Dr Renton, had also seen the eye, and was much of the same opinion with myself. He accordingly took her to the New-Town Dispensary in Edinburgh, for advice, and was happy to find that Dr Thomson, and Mr Turner and Mr M'Lagan, the two surgeons to that useful institution, coincided with him in opinion, that the disease ought to be removed as soon as possible by the knife.

On the second of August I performed the operation, in the presence of Dr Thomson and Mr Turner. An incision about an inch long was made by a pair of probe-pointed scissors, through the external canthus, to allow the free dissection of the tumour. A common scalpel was now used; the dissection being carried from the first incision downwards, and upwards to the puncta lachrymalia. Another semicircular sweep was then made along the upper part of the tumour; the upper eyelid being divided for the same purpose. The two semicircular dissections being finished, and the adhesions of the tumour to the lateral and posterior parts of the orbit being removed as far back as possible, a strong curved needle, armed with a broad ligature, was next passed through the body of the eye. A strong small scalpel, slightly curved towards the extremity, was moved steadily round the sides of the tumour upwards and downwards, and, by gradually raising and depressing it in these directions, the diseased mass was speedily extirpated. The tumour seemed to be sacculated, and the sclerotic coat to be thickened, and to form with the sac a strong covering, which was afterwards dissected from its adhesions to the internal orbital plate. The discharge of blood from the internal orbital artery was very trifling, and was easily suppressed by the introduction of a piece of sponge into the orbit. The usual dressings were applied, and secured by a roller round the head, and the patient was put to bed. On the fifth day after the operation, the sponge was removed, and a slight hæmorrhagy occurred from the extremities of the blood-vessels, which had been adhering to the surface of the sponge. The wound was regularly dressed, and the pus it discharged, which was never abundant, appeared always to be remarkably healthy. By the end of seven weeks, the cavity of the orbit was almost filled up with healthy granulations, which have only twice required the application of the nit. argent.

The appearances of the eye, upon dissection, afforded a strong proof of the accuracy of Mr Wardrop's opinion, "that the disease consists in a morbid change of the nerve itself." The optic nerve seemed sound where divided, but at the place where it passes through the sclerotic coat, and expands into the retina, there was a copious intermixture of a substance similar to that composing the tumour, producing a considerable thickening at the origin of that membrane. The retina itself was thicker and firmer than usual, and adhered to the choroid coat, and the diseased growth appeared intermixed in its substance. The tumour consisted of a diseased growth surrounding the eye-ball, between it and the muscles, attached to the tunica sclerotica. The substance of the tumour was of a dark brown colour, passing into a brilliant black at some points, and of a flocculent texture. The muscles, and cellular substance of the eye-ball, which were removed, appeared healthy, and no detached portion of disease could be found among them.

The result of the case, so far as it is allowable at present to judge, promises to be fortunate. It presented two circumstances, the incipient state of the tumour, and a naturally strong and unirritable constitution of the patient; advantages, without which it were vain to perform an operation for this most unmanageable disease with any probable chance of success. She bore the operation with much fortitude, and had no attack from secondary fever. She is of a sanguine temperament, and possessed with that particular disposition of mind, (a circumstance much to be wished for in those who have to undergo such painful operations, and which I have no doubt contributed not a little to her recovery,) which led her fearlessly to embrace any expedient suggested for her benefit, without calculating on the dangers of its consequences. The lancinating pain, which extended to the occiput, and which had continued with few intervals of mitigation for thirteen months, became much less severe upon the removal of the sponge on the fifth day after the operation, and gradually decreased, so that, in the space of a month, she could press strongly with her hand upon any part of her head, without occasioning uneasiness. Her health daily improved, and she now takes more exercise than she has been able to do, without producing the same fatigue, for many months previous to the operation. She thinks she sees more distinctly with the right eye, although she was never sensible of its vision being impaired. The under eyelid, which was of a livid and diseased hue, has now acquired its natural colour, size, and appearance, so that there is every reason to believe from this circumstance, and from the entire absence of pain, as well as from the healthy appearance of the wound, (the superior and inferior eyelids being completely united,) and from the patient's restoration to health, that the progress of the tumour has been arrested in its incipient state, and that neither the soft parts adjacent to it, nor the bones which compose the orbit, had been contaminated by the disease.

Penicuik, December 9, 1818.

III.

Copy of a Quarterly Report of the Royal York Rangers, at Trinidad, for the quarter ending the 24th December 1816. Transmitted to the Army Medical Board, through the Inspector of Hospitals in the West Indies, by JAMES M'CABE, Surgeon to the Royal York Rangers.

SINCE the last report, the weather has been more favourable in this island than it had been since the arrival of the regiment; the consequence was, that there was less acute disease among the men, but the number labouring under chronic affections sufficiently swelled the list of sick, and kept it up to a most melancholy extent. Since the beginning of this month, however, the rainy weather has again commenced, attended, as usual, with a chilling unhealthy atmosphere; it has occasioned an increase of acute disease, and the old worn-out cases also drop off in a greater proportion than during the two preceding months.

A case of fever, now in hospital, and I believe likely to do well, has been very interesting, on account of an uncommon symptom that attended it. In the course of the disease, a considerable degree of inflammation was observed in the nose; a solution of acetate of lead was applied to it, but in a few days it was found that the spongy bones were destroyed, and the cavity filled up with maggots of the common size, and such as are usually generated in putrefied animal matter. Hundreds of these maggots were taken away, but their places were again supplied by a succession of others; their source seemed inexhaustible. They appeared to be supplied from the antrum and other cavities of the face. It was apprehended, that a caries of the whole of the bones of the face would take place, as a considerable degree of fetor attended it, and in the progress of the disease became intolerable. Charcoal and camphorated spirits were applied to remove the fetor, and the spirits of turpentine, snuff, and the powder of an acrid vegetable that grows in the island, called by the inhabitants "Capuchin powder," were successively tried to remove the maggots. Camphor, opium, porter, &c. &c. were given internally. The progress of the caries stopped with the destruction of the palate bones, and spongy bones of the nose, and the maggots have now ceased to be generated, at least to be discharged. The inflammation of the surrounding parts is removed, and although there are several holes in the upper part of the nose, occasioned by the destruction of the bones and soft parts, the patient is likely to recover.

I am at a loss to know, whether the maggots in this case might have been generated by insects finding their way into the cavities of the face, and there depositing their ova, and that the caries of the bones was a consequence of the irritation occasioned by these insects; or whether the impure air of a crowded hospital, rendered still more impure by the miasmata arising from a swamp in its immediate vicinity, might have, by its effects on a diseased constitution, occasioned the caries, and that these insects were generated by the progress of disease among the bones.

In the treatment of fevers the same practice has been followed which was mentioned in the former report, and attended with considerable success. The lancet was used whenever the disease was attended with any degree of increased arterial action, and particularly when attended with pneumonic inflammation, which has been the case with many. The low fevers were treated with calomel purgatives, camphor, calomel, James's powder, opium, &c. according as symptoms appeared to require. The acute dysenteries were treated with bleeding, neutral salts, pulv. ipecac. comp. c. opii, and in the more aggravated forms the system brought under the influence of mercury. In the cachectic and chronic cases of disease, calomel, combined with opium, along with such other remedies as appeared likely to relieve the most urgent symptoms; digitalis, tinct. muriat. ferri, colombo extract. cinchona, &c. according to circumstances and prevailing symptoms.

That disease which has so frequently been spoken of, and considered peculiar to Trinidad, and from which the Royal West India Rangers suffered so much at this post, is now well marked in many of our men. Their faces are bloated, cadaverous, and dejected; feet and legs œdematous; respiration hurried and difficult. They become exhausted by the least fatigue. Some of them are observed, even during the gentle exertion of walking, to stop suddenly, and, laying their hands on their knees, endeavour, by bending the body, to assist their laborious breathing. This disease has, I understand, been considered a disease of the stomach, and called "Mal d'Estomac." In its form among our men, although it is always attended with sickness and nausea, the affection of the stomach appears to be usually a consequence of a generally diseased state of the system. When the symptoms I have mentioned have been present for some time, the unfortunate sufferer either becomes anasarcous, or a wasting diarrhœa attacks him, or perhaps, when received into hospital, he only complains of difficulty of breathing, which in these cases is occasioned by water in the chest, or perhaps a complication of all these symptoms will be present in the same individual. If relief be afforded him, and he is again discharged from hospital, it is only a temporary respite; he is again admitted with more aggravated symptoms, and his constitution at length sinks under this complication of diseases.

To trace a European soldier from the time he arrives in Trinidad in perfect health, till his constitution is exhausted by a succession of diseases, is melancholy but interesting. A post like St Joseph's, in the middle of an island, and without any barrier or boundary to restrain the excursions of the soldier, or to prevent the introduction of rum, affords every facility of indulging in this destructive liquor, particularly as the barracks are surrounded with sugar plantations, where new rum can be got for a mere trifle; the new soldier or recruit greedily indulges in this, and is frequently, while under the influence of excesses, exposed to the chills and damps of the post. After each monthly payment he goes into hospital with a bowel complaint, or perhaps an attack of fever. A succession of these seems to recur with the monthly periods of payment. At length the habit, which was robust and plethoric on his first arrival, becomes cachectic and œdematous, water is now contained in the chest, and perhaps the abdomen; the lungs frequently diseased; the stomach rejects food; and the whole constitution has now suffered that change which characterizes the disease of the island.

In producing this change in the constitutions of the men, there are many other causes that might be mentioned. The bad state of the barracks, and the marshy nature of the country, have been represented in a former report. A great cause among soldiers of a certain description, is the insuperable barrier which they know is opposed to their return to their own country. When hope is cut off, desperation succeeds to it; and viewing their situation through the gloomy medium of despair, they become careless of existence, and consequently indulge in any excesses that produce a momentary gratification.

More particular Account of the Mal d'Estomac, with appearances on Dissection.

THE preparation herewith transmitted is the heart of a man, who died a most marked case of that disease, which has been called in this island "Mal d'Estomac," but which may more properly be considered the effect of a complication of disorders, inducing a cachectic habit and a broken constitution. A soldier at this station, after having had several attacks of fever, remittent and intermittent, and probably also several attacks of dysentery, after each of which his constitution had been visibly impaired, becomes of a pale and sallow complexion; his feet and legs swell, particularly in the evenings; his stomach and bowels are generally out of order; he has frequent palpitations; and walking up an ascent occasions a sense of suffocation. In the more advanced stages his stomach rejects food; the cellular membrane over the body is distended with fluid, and being almost transparent, exposes to view a peculiar white adipose substance, deposited in the cells of the cellular membrane. The breathing becomes more difficult, and the least exertion occasions a sense of faintness. The face is bloated, and the eyes haggard and ghastly; the pulse small and unequal. These aggravated symptoms are not present until after a patient has suffered from several attacks of disease; but a certain approximation to this state is easily observable after each imperfect recovery, and the constitution appears, after each successive attack, to be rendered less capable of effecting its own cure.

Intemperance may be considered one of the principal causes of this disease, or rather of this complication of diseases, and when the changes above mentioned have taken place, the unfortunate sufferer becomes more infatuated than ever with the love of ardent spirits. The uncomfortable sensation which he experiences in his stomach, he imagines, is relieved by the stimulus of rum, and as it probably produces a temporary alleviation of his mental and corporeal sufferings, no consequences can deter him from indulging in the most dangerous excesses.

The most remarkable circumstance that presents itself with respect to this preparation, is its great want of muscularity, and the quantity of fat deposited about its base. The ventricles, instead of being firm and muscular, yield on pressure, and their sides close together like a pouch or membranous bag. This great want of muscularity is not now so marked as it appeared in the subject at the time of dissection; it has now been preserved during several months in strong spirits, or what is called by the planters "high wines." The corrugating effect of the spirits has given to the preparation some appearance of muscularity, of which, at the time of taking it out of the subject, it appeared to be totally destitute.

The following were the appearances on dissection of the subject to which this morbid preparation belonged. Surface of the body white; cellular membrane distended with fluid, and almost transparent. An incision made along the inner part of the thigh with a scalpel, was followed by a copious flow of a watery fluid; the cells of the cellular membrane filled with fat; a quantity of fluid between the dura and pia maters; a preternatural quantity in the ventricles of the brain and base of the skull. The whole cavity of the chest filled with water. The heart deprived of its muscularity. About one ounce of a yellow substance of the consistence and appearance of jelly in the right auricle. A quantity of dark-coloured coagulated blood in the same auricle. The liver indurated, and of the colour and consistence of new cheese.

To deduce any consequences from this remarkable state of the heart, connected with cachectic diseases, appears difficult. It may not, however, be uninteresting to consider some of the principal causes which may be supposed to occasion this deranged state of the system, and to see how far they are capable of producing the morbid changes above mentioned.

The stimulus of spirits taken into the stomach occasions an increased action in the heart and arterial system, by which the blood is propelled into the extreme vessels, producing diaphoresis on the surface. A soldier, while under the influence of excesses, frequently exposes himself, at this post, to the damp and cold chills of the night. The difference of temperature is here very great, at different periods of the day and night. A man under the circumstances just mentioned, who has been exposed during the day to a temperature, even in the shade, that is frequently as high as 86 degrees, is particularly liable to be chilled by a damp and wet atmosphere of about 62, to which the thermometer frequently sinks here at night, or about gun-fire in the morning. This damp and cold air probably occasions a reaction in the vessels on the surface, and the blood retires from the extreme vessels to the heart and other internal organs. The blood driven on these organs occasions a local congestion, and the watery parts of it being carried off by the exhalants, an inflammatory diathesis is occasioned. Is it not probable that the excessive action of the heart, occasioned and continued by the causes already mentioned, and directed on itself by the reaction on the surface, is the cause of the destruction or absorption of its own muscular structure? I have seen in some subjects that had died of this disease, a layer of coagulated lymph, connecting the surface of the heart to the pericardium so firmly, that it required a considerable degree of force to separate them. This circumstance appears to afford a proof of the previous existence of inflammation in the heart, for although this lymph might have been effused from the vessels of the pericardium, we can scarcely suppose the existence of an inflammation in the pericardium without its having extended to the organ it surrounds.

By whatever process the muscularity of the heart is destroyed, the fact of its being so is proved by this preparation. The first impression suggested by its appearance on dissection was surprise, that a heart, so destitute of muscularity, could have given to the blood the impetus necessary to carry on the circulation. The heart, thus deprived of its muscular structure, is unable to propel the coloured part of the blood into the extreme vessels, the consequence of which is, that paleness and transparency of the skin is remarkable. The languid circulation is also probably the cause of the great deposition of fat which takes place in the cellular membrane over the whole surface of the body, and gives to the skin that white and flabby appearance. With respect to the gelatinous substance and coagulated blood found in the right auricle, I can only hazard a conjecture; from the languid action of the heart in the last stages of the disease, it is probable that the auricle, during its contraction, could not completely empty itself of the whole of the blood received from the cavæ, but that a certain portion remained in contact with its sides, which was afterwards increased by the accession of more, as the circulation became more languid, and that the gelatinous substance was formed by the decomposition of the stagnated blood, or by an increase of the natural heat of the heart occasioning coagulation.

IV.

Case of Tetanus, in which a large quantity of the Tincture of Opium was administered by mistake. By John Hall, M. D. Physician in Berwick. Communicated, with some Prefatory

Remarks, by Dr Dickson, F. R. S. Edin. & L. S. Fellow of the Royal College of Physicians of Edinburgh, and Physician to the Clifton Dispensary.

Dear Sir,—I send an account of a case of tetanus, communicated to me by my friend, Dr Hall of Berwick, where the spasms were removed by the patient having taken about two ounces of the tincture of opium in the course of the night, by mistake, and in which the advantage thus gained was followed up by an active and judicious perseverance in purgative medicines, aided by opiate frictions, and afterwards by the cold affusion.

In the favourable effect of these means, we see an example of the greater mildness of the idiopathic species, by comparing it with those fatal cases of traumatic tetanus which I stated in the 7th Volume of the Medico-Chirurgical Transactions, to have occurred off the Mississippi, in one of which, a still larger quantity of laudanum was exhibited, without arresting the rapid progress of the disease.

Besides the free use of opium in the present instance, Dr Hall very properly lays much stress upon the liberal employment of purgatives to obviate the state of intestinal torpor, which is so constant an attendant, as well as so general a precursor of tetanus; and, independently of the testimony of preceding writers, I am gratified to find my former remarks on this head farther confirmed by such authorities as Sir James M'Grigor, Dr Morrison, Dr Johnson, M. Boyer, M. Vialle, and Mr Hennen. The conclusions which my limited observation of the disease had led me to form also respecting the great fatality of acute symptomatic tetanus, as it occurs in our armies and fleets, are fully corroborated by the result in "several hundred cases," as stated in those excellent practical works, which are the fruit of the very ample experience afforded by the peninsular war. Upon these points Sir James M'Grigor remarks, "There is throughout obstinate constipation of the bowels which require the most drastic purgatives, with a great sense of uneasiness about the precordia: the tongue is always moist. The remedies which have been chiefly trusted to for the cure of this formidable disease are opium, mercury, wine, warm and cold-bath, venesection, ipecacuanha, and digitalis, in large doses, enlargement of the original wound, and amputation of the limb. These have been tried alone, and in various combinations, and I am obliged to confess, that the whole failed me in almost every acute case of tetanus which occurred. The three first have been administered in unlimited doses without effect."* Mr Guthrie thus characterizes tetanus: "A disease, in its acute form, remediable by no operation or medicine that I am acquainted with." p. 235. And, speaking of amputation, he adds, that it is a very violent attempt to remove a disease that few will submit to in the mild tetanus, and which was useless in the acute in all the cases in which he tried it.†

Mr Hennen also, in his late and very able work on Military Surgery, says, "I have, in short, observed no symptom among the great number detailed by writers on this disease invariably present, except costiveness." p. 265. Again, "I have never been fortunate enough to cure a case of the acute symptomatic tetanus. In some instances of the chronic species I have effected or witnessed relief. I shall not take up my readers' time by detailing my disappointments; they embrace almost, if not every remedy that has come within the knowledge of practitioners." p. 262. These remarks are not quoted with the view of discouraging, but, on the contrary, of evincing the necessity of attempting, by correspondent energy of practice, to counteract the earliest approaches of this formidable disorder; and, consequently, the propriety of distinguishing between the degrees, or what may be called the acute, subacute, and chronic forms of tetanus; for it is by such discrimination only that we can reconcile the difference of opinion that has been entertained of its danger, and of the value of different remedies, or hope to proportion the activity of treatment to the power of the disease.

These results farther show the importance of cultivating every opportunity of elucidating the ratio symptomatum, and proximate causes of tetanic affections by dissection, and particularly by examining the state of the spinal chord and its membranes. For as a principal function of the medulla spinalis is to excite the muscles of voluntary motion, it is a fair conclusion, that this most important part of the nervous system is itself suffering from irritation, when these muscles are stimulated to violent spasmodic action; and on mentioning this idea to the author last quoted, (as is noticed by him, p. 267,) as sanctioned by farther reflection on the phenomena of tetanus, I was gratified to find it in conformity with his own opinion, and with the light recently thrown upon the pathology of the spine, by the observations and investigations of Drs Franck, Sanders, and Reid, and other able men who are now prosecuting this interesting branch of morbid anatomy.

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* Medico-Chirurgical Transactions, Vol. VI. p. 453.

† Guthrie on Gunshot Wounds, p. 258.

Agreeably to the experiments of M. Le Gallois, the spinal can perform this office of exciting the voluntary muscles perfectly, independently of the cranial brain; yet, as the sensorial naturally influences the operation of the spinal, or nervous system, and from the intimate connection and sympathy subsisting between them, the brain might also be expected to exhibit evidence of irritation in many instances; and, accordingly, besides the more prominent disease of the spine, signs of cerebral affection were discovered in the cases detailed by Dr Reid, and by some of the authors whom he has adduced. Dr Abercrombie, likewise, in his excellent pathological observations in the 55th Number of the Edinburgh Medical and Surgical Journal, mentions, that " in a case of tetanus described by Le Cat, the most remarkable appearance was an evident inflammation of the pia mater, with some appearance of suppuration." Other examples might be added.

It is, however, as already observed, to the spinal marrow, and its investments, as the grand source of the nerves of the voluntary muscles, that attention has been recently directed, as the principal seat of this complaint; and it is peculiarly interesting to learn from the above and other late authorities, that those parts have manifested striking appearances of irritation and disease in persons who have died of tetanus.

An instance, extracted from the Journal de Medecine, is recorded in Dr Johnson's Medico-Chirurgical Journal for July 1818: " M. Dupuytren lately opened a man who died of *traumatic* tetanus, exquisitely marked, where the coverings of the spinal marrow were intensely red, which redness extended to the coverings of the nerves as they emerged from the theca vertebralis." p. 118. Two cases have also occurred lately to Professor Brera of Padua, which are noticed in the London Medical and Physical Journal for September last. The first arose without any evident cause in a woman, who died on the 15th day. " On dissection, a considerable quantity of pus was found in the cavity of the abdomen, the consequence of general peritoneal inflammation; the spinal marrow was soft, and considerably altered in structure; there was effusion of serum between its coats, and the vessels were much distended with blood." The subject of the second case was a young man, in whom the disease had been induced by a contusion on the thumb of the right hand. " Professor Brera directed 120 leeches to be applied along the course of the spine, and prussic acid to be exhibited internally. After the leeches had been applied, relaxation of the muscles came on; but this was succeeded by paralysis, and he soon afterwards expired. The examination of the body after death was highly interesting; there had been violent inflammation of the spinal marrow, commencing at the situation of the lower cervical vertebræ, and continued to the extremity. But what is very remarkable, the inflammation was confined to the right side of the spinal marrow, corresponding in this respect with the hand injured." p. 259.

Mr Baynton, who has instituted considerable improvement in the treatment of diseased spine, informs me that he has met with two instances of this complaint, succeeded by subacute or chronic trismus. The spinal column generally was affected, but particularly the cervical portion; and it is extremely deserving of remark, that in each the inflammatory condition extended to the brain, producing temporary insanity. The lower limbs became paralyzed in both instances, and in one the upper extremities were also affected in a less degree.

One of these patients, who could not be induced to submit to absolute recumbency, died with locked jaw; the other, (a young lady, whom I have seen since,) under this mode of treatment perfectly recovered. As I understand from Mr Baynton, that he has met with a great many cases, which he considers as having fully established his former opinions, since his valuable work on the diseases of the spine was published, it is to be hoped that he will take an early opportunity of laying the results of his farther experience before the public, in a new edition.

Enough has been quoted to attest the importance of examining the state of the spinal canal in fatal cases of tetanus. The growing attention to its pathology, we may now hope, as I remarked on another occasion, will throw increasing light on morbid phenomena, which are the most obscure and least understood, not in this only, but in various other diseases, and even in fever itself. This, however, opens far too extensive a field to enter at all upon here; indeed, lest there should appear to be any interruption of continuity, or repetition in the remarks offered in this paper, it may be proper to observe, that the greater part of it was written some time ago, and that the above *post mortem* appearances having been more recently promulgated, have been added since it was sent for publication, as decisively proving that the principal object of attention in tetanus, is the nervous mass of the spine and its membranes.

The effects of the nux vomica, as an excitant of the spinal marrow, upon which the species of strychnos act, and its power of inducing convulsions and symptoms resembling those of tetanus, as stated by M. Orfila and Fouquier, are also strongly in favour of this opinion. I have gradually increased the dose of that very powerful medicine recently to five grains of the extract in one case of hemiplegia, and to seven grains in another, twice a day, (not, indeed, with the same success as M. Fouquier, for they were not the kind of cases in which he has been successful, but certainly with advantage,) and in the latter case, particularly, it produced the prickling sensation in the affected limbs, startings, and very similar effects to those described by him. * When augmented to seven grains the startings were greatly increased after each dose; the lower extremity was rigidly extended, the upper extremity was contracted and drawn close to the breast. The muscles of the neck and jaw became rigid, and the head was drawn backward; and if the dose had been carried farther, I have no doubt complete tetanus might have been induced. How far we might, *a priori*, expect the involuntary muscles to be affected in the worst form of tetanus, I must leave to others more competent to decide. Reflecting upon the difficulties attendant on the investigation of the relation of the nerves with the heart, and upon the series of connections between the different nervous centres or ganglia, considering how much has been attempted in this path of inquiry, yet how much remains to be ascertained, and, that we are unable to establish any limited laws as to the phenomena arising from diseased nerves, it is with much diffidence that I even advert to this obscure branch of physiology, from a sincere desire to suggest any thing that may, in abler hands, tend to elucidate the pathology of tetanus. I therefore solicit the indulgence of remarking, that, as M. Le Gallois maintains that the heart derives its power from the spinal marrow, and as the experiments of Dr Philip, though they tend to confirm the doctrine of Haller, that the heart possesses an independent power, refute that part of it which teaches that the nervous system has no direct influence over the muscles of involuntary motion, by shewing that the heart is not merely capable of being stimulated through it, but of having its power wholly destroyed by powerful agents acting on the system; it does not appear to me unjustifiable from these data to infer that, in this way, the heart, and, by the same reasoning, the intestines, may be occasionally expected to manifest signs of morbid affection in dissections of tetanus. But whatever may be the propriety of such theoretical reasoning, that they have done so in some instances will appear by the following examples, which will suffice to illustrate the expediency of examining those parts after death.

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* Vide Medico-Chirurgical Journal for November 1817, p. 416.

In the 39th and 43d Numbers of the London Medical Repository is noticed a case of tetanus by M. Patissier, caused by pricking the great toe with a nail, when the patient died in five days. On dissection, " the membranes lining the cavity of the spine, and the internal surface of the heart, presented a highly inflammatory aspect."

Relative to the contraction of the heart, I regret that I cannot at present refer more particularly to the observations of Mr Howship, in the 22d Volume of the London Medical and Physical Journal; but I observe, in a subsequent Number of the same work, that, in the opinion of the Reviewer, he is considered as having satisfactorily proved, that the fatality of tetanus depends on the muscles of the heart partaking of the contraction. In one case, Mr Howship found the contraction so firm, as almost to resist the knife. Dr Parry, in his Treatise on this disease, and Rabies contagiosa, is of opinion, that the heart becomes exhausted, and sinks from the loss of its vital power in tetanus; and he gives a dissection of hydrophobia, analogous to Mr Howship's observations in a tetanic case. " The heart was extremely hard, and strongly contracted; having in its cavities a very little clotted blood."—See also the Medical and Physical Journal for April 1815, p. 316.

Indeed, Dr Currie observes, that, in tetanus, the patient probably dies at last from the spasmodic affection extending to the heart. With respect to the state of the intestines, Dr M'Arthur furnished me with four cases, with dissections, which have already appeared, where they exhibited extensive proofs of inflammation. In the second case, there were also " strong marks of inflammation, both in the internal and external coat of the stomach;" in the third, " stomach much drawn up under the diaphragm;" in the fourth also, this viscus was " singularly contracted, the internal coat exhibiting appearances of inflammation;" but in all of them, the intestines were found much inflamed. *Vide* Medical and Chirurgical Transactions, Vol. VII. p. 462.

I observe there is a similar instance in the Edinburgh Medical and Surgical Journal, Vol. VI. p. 173, translated by a naval surgeon, from the Medecine Pratique of Stoll, in which, on dissection, " the jejunum was of a livid red colour, and contained some lumbrici. The colon, to the extent of a span, just before it terminates in the rectum, exhibited its vessels larger, and more numerous than usual. They were all of them of a very red colour; some of them livid. A large proportion of the mesentery to which this intestine adheres was of the same colour." Larrey also found numerous lumbrici in several patients who died; and, though not actually the cause, they may be supposed to increase the tendency to inflammation.

Finally, it is here deserving of notice, that, not long ago, I received a letter from Dr Douglas, of the Argonaut hospital ship at Chatham, mentioning,—and he was probably unaware of its coincidence with the above, that he had recently witnessed a case of tetanus, in which "the only extraordinary appearance on dissection was a large part of the intestinum ileum being in a highly inflamed state. Upon opening it, a very large vermis lumbricus was found dead; and a second in the caput cæcum coli." I regret that I have not received any further particulars of this case; but I believe the spine was not examined. Without offering any farther remarks on this interesting subject at present, I shall proceed to relate the case with which I have been favoured by Dr Hall, nearly in his own words.

"Case.—George Fortune, aged 33 years, of ordinary stature and robustness, married, and of sober industrious habits, employed in husbandry on a farm near Coldstream, and works a pair of horses. 10th May 1809.—Complains of severe pain in his breast, with difficulty of breathing, but no cough. When he attempts to swallow, the pain is considerably increased; the pulse is 96, hard and full, the bowels costive, and tongue loaded; but no particular thirst. He says he was much heated and fatigued, in preparing some land, and that he has taken cold.

Statim detrahatur sanguis e brachio ad ℥xvi. et capt. quam primum P. Jalapæ comp. ʒiss.

11th.—The blood drawn yesterday shewed no signs of inflammatory affection, nor has it relieved the pain, which, upon closer examination, appears to extend down the sternum to the end of the xyphoid cartilage, and through to the spine. He has had only one large evacuation from the cathartic.

Applicetur pectori emp. meloës vesicatorii.
℞ Aquæ menthæ pip. ℥j.
Spt. æth. nit. ʒj.
Tinct. opii gutt. xl. Fiat haustus hora somni sumendus, et capt. cras mane bolum ex submur. hydrarg. gr. vi. pulv. jalapæ gr. xxv Zinzib. gr. iv. M.

12th.—The blister has risen, but does not seem to have relieved the pain. He says he felt easier for two hours after taking the draught: purge operated twice; the pulse is 92; the tongue loaded; has more thirst; and finds the pain and difficulty of swallowing liquids increased.

℞ Aq. acetatis ammoniæ ℥ij.
Mist. camphoræ ℥iv.
Tinct. castorei ʒij.
Ætheris sulphurici ʒj.
Tinct. opii gutt 100.
Syr. simplicis ℥ss. M. Sit dosis cochlear. tria 2da qq. hora.

8 P. M. The pain is not relieved; and he is much convulsed on swallowing liquids.

℞ Pulv. Ipecac. comp. ℈j.—H. s. utatur pediluvio.

13th.—He slept better last night, and perspired freely; pulse 88, no stool.

Rep. bol. purg. cum sub. hydrarg. gr. viii.

14th.—He has not been able to lie down in bed from the violence of the pain at the bottom of the sternum, and is much convulsed. When he attempts to swallow, the extensor muscles of the neck and back act so strongly, that his head is bent quite back. The pulse is 94; tongue loaded; the purge operated only once.

Cont. mist. antispasmodica. Habeat q. p. enema domesticum. Admoveatur inter scapulas emp. cantharidis; Rep. pediluvium, et pulv. ipecac. comp. ʒss. h. s.

15th.—He slept none last night; the skin is hot and dry; the pulse is 96; and all the tetanic symptoms are more violent. It was in consequence proposed to his friends to call in the assistance of Dr Douglas of Kelso. Upon visiting him in the evening with Dr Douglas, we found every symptom much aggravated. He was obliged to sit up, supported by two assistants, who with difficulty could keep him in bed when the spasms came on. His pulse did not exceed 94, but his features were much altered; and whenever the spasmodic affection returned, it was preceded by the appearance of spasmus cynicus. In consultation, it appeared advisable to try tepid immersion; but being unable to obtain a convenient bath, we were obliged to continue the pediluvium, and although the necessity of administering opium was strongly indicated, it appeared equally necessary to try the effect of powerful purgatives. With this view, we ordered the following cathartic electuary, and anodyne embrocations.

℞ Submuriatis hydrargyri, ʒj.
Gambogiæ ℈j.
Pulv. Jalapæ ʒij.
Elect. Sennæ ʒj. M. Fiat elect. cujus capiat magnitudinem nucis moschatæ omni hora, donec alvus plene dejecerit.

℞ Tinct. opii ℥ijss.
Æth. sulphurici.
Sp. camphor. ā ℥ij. M. Fiat embrocatio, cujus illinatur semuncia in part. dolent. secunda quaque hora.

16th.—By a very singular mistake, notwithstanding the written directions sent with the medicines were very correct, it appeared that the persons entrusted with their administration had completely reversed them; and instead of giving him a tea-spoonful of the electuary every hour, and rubbing in a table-spoonful of the embrocation every two hours, they had caused him to swallow a table-spoonful of the embrocation every two hours, and had rubbed the electuary upon his breast. Upon visiting him this morning at 9 o'clock, we were surprised to find, that one drachm of calomel, a scruple of gamboge, and two drachms of jalap which we supposed had been taken, had produced no effect upon his bowels, until we discovered that it had been used externally; but we were still more surprised to find, that the whole of the embrocation, with the exception of very little more than one spoonful which remained, had been swallowed in the course of the night, in which time he had taken nearly two ounces of the tincture of opium. The æther may have been usefully combined with it; but, as it was intended for external use, that and the camphorated spirit were added merely to disguise the laudanum. The spasms at the period of our visit were perfectly relieved; but the constipated state of his bowels made it more necessary than ever to follow up the proposed plan of free purgation. We therefore ordered the electuary to be repeated, and the opiate frictions on the scrobiculus cordis, as originally intended, at 5 P. M. He had taken the last dose of the electuary without effect, but the spasms were so much relieved, that he was out of bed, sitting in a chair. The following purging solution was ordered.

℞ Sulphatis magnesiæ ℥iij.
Aquæ fervidæ lb. ij. Solve. Capiantur ℥iv. cum partibus æqual. seri lactis vaccin. omni hora.

8 P. M. He has had three stools; pulse 86, and softer. Cont. sol. purgans.

17th.—He had three more evacuations during the night, *dark-coloured and fetid;* he did not sleep much, but was free from spasms. The sardiasis is still particularly striking when he swallows, which he says still gives him pain. Cont. solutio, et frictio.

18th.—Visited him with Dr Douglas. He has had no return of spasms, but the pain under the sternum and ensiform cartilage is so severe, as to threaten a recurrence of the tetanic affection. The purging electuary and sulphate of magnesia to be continued; and as soon after their operation as convenient, to try the cold affusion.

19th.—His medicines operated very freely; he feels a little better, and has some inclination for food.

20th.—He has had slight returns of the spasms, but they did not continue long; has not yet tried the cold affusion.

21st.—He feels better, but has had no stool since the physic; the embrocation seldom fails to relieve the pain. Cont. frictio, necnon repet. sol. cathartica.

22d.—He had two evacuations yesterday, but has not yet used the cold affusion, to which he appears to have a reluctance, but promises to begin to-morrow. From this period, I did not see him for nearly a fortnight, being confined to the house by a severe accident; but he was reported to be doing well, and to be using the shower-bath;

and by the time I was able to revisit him, I found him walking about."

"To which of the remedies," Dr Hall continues, "are we to refer the recovery of the patient in the case before us? Is it to be imputed to the free use of purgative medicines, or to the accidental exhibition of so large a quantity of laudanum? There can be no doubt that the progress of the disease, in the first instance, was arrested by the opium, which allowed time for the administration of other medicines; but, from the subsequent threatenings, which he had, both on the 18th and 20th, when the sedative effects of the opium had ceased, I am inclined to think that unless the plan of powerful purgation had been followed up, there would have been much difficulty in subduing the disease. Even after the severer symptoms had ceased, the spasmus cynicus and pathognomonic pain under the xyphoid cartilage did not subside till after repeated and severe purging; and the case certainly is in favour of your opinion, that a torpid state of the intestinal canal may give or increase the predisposition to this disease, and of the utility of purgatives both in a prophylactic and curative point of view."

"Is it probable that the mercurial friction, in the shape of the calomel electuary, unintentionally employed, had any effect? To this I should be inclined to give a negative; as, during the whole period of the cure, notwithstanding the quantity of calomel given internally, as well as that externally applied by accident, no affection of the mouth took place, to induce us to suppose that a quantity sufficient to counteract so formidable a disease could have been introduced by this means; for if it had, we should have availed ourselves of the hint, and have thrown in a proper quantity to produce its specific effect. With respect to the cold affusion, not having witnessed its application, I cannot speak as to its effect, nor was it used during a paroxysm; but if there had been any occurrence of the spasms, I had determined upon giving it a full trial. It seems highly probable, that, if bleeding had been carried farther in the present case, great benefit would have been derived from it. I was at particular pains to investigate, both at the time and subsequently, whether my patient had previously met with any wound or other injury; but he did not recollect any such accident; and imputed it entirely to exposure to cold, after being fatigued, and overheated, for the weather was unusually sultry. Whether these were the remote causes in the present instance, I cannot take upon me to say; but were such causes alone adequate to the production of the disease, we would meet with tetanic affections much more frequently. It is, therefore, probable, that there existed a constitutional predisposition here, which rendered him liable to be affected upon the exciting cause being applied; and from the pain about the præcordia, to which he always referred, that some local injury had been formerly sustained, and which, under the excitement of the vascular system, produced by unusual heat and exertion, acted upon the nervous system by sympathetic affection."

"The jaw was not at any period so far closed as to prevent the administration of medicine, but the spastic rigidity of the muscles and difficulty of swallowing increased so much on the 14th and 15th, that it is probable complete trismus would have supervened, if it had not been anticipated by so powerful, but unintentional, an exhibition of opium."

V.

Observations upon the Action of Mercury, in the Cure of Venereal Diseases, intended as a Reply to those of Professor Thomson, contained in the 53d Number of the Edinburgh Medical and Surgical Journal. By JOHN BUTTER, F. L. S., M. W. S., Member of the Medical Societies of London, Edinburgh, Paris, &c.; Member of the Royal College of Surgeons, in London; Surgeon to the South Devon Militia, at Plymouth. Communicated to Dr DUNCAN, jun.

DEAR SIR,—The great practical question which constitutes the present inquiry, viz. whether or not all kinds of venereal diseases can be cured effectually and safely without mercury, has become a subject of such general interest, that I wonder reports are not more frequently laid before the public, with the view of determining, by comparative evidences, if mercury be any longer necessary in the cure of syphilis.

In the number of your Journal already alluded to, there is a communication from Dr Thomson, Professor of Military Surgery in the University of Edinburgh, shewing "that all manner of cutaneous eruptions and ulcerations, ulcerations of the throat, pains and swellings of the joints and ligaments, and nodes of the bones, gradually disappear under its (sarsaparilla) mild operation, when its use was duly persisted in, and was, at the same time, accompanied by attention to regimen, and the proper local treatment." (page 85.)

There can be no other inference drawn from this quotation, than that every secondary or constitutional symptom of syphilis can be cured without mercury; and if such be really the case, great good must accrue from the discovery. As this opinion is somewhat new to me, I am disposed to inquire into its validity, especially as I have seen no confirmation of it in the subsequent numbers of your Journal.

In the first place, I am disposed to be rather sceptical, as no cases are given in proof of the identity of syphilis, nor is any mention made of the sort of sarsaparilla used. After Sir William Fordyce had introduced this root into England, many persons made experiments to determine the power and extent of its medicinal virtues in the cure of several diseases, and its inefficacy was proved in real syphilis, whilst it was said to benefit some forms of pseudo-syphilitic affections. In revolutionizing established notions of practice, one cannot be too clear in adducing facts, wherewith to build general principles. It must occur to every one to ask Dr Thomson, what his proper local treatment means, if it consisted of mercurial salves, lotions, &c.; because mercury may be made to produce its specific influence in the constitution through the medium of sore-surfaces.

In another part of the same memoir, Dr Thomson observes, "In this hospital, (meaning the consolidated depot hospital in Edinburgh Castle,) open to the inspection of all military medical officers attending the University, I have, since that period, (March 1816,) carefully abstained from the employment of mercury, not only in the treatment of secondary, but also in that of the primary symptoms of syphilis, and have found, that chancre and bubo have, *in every instance*, disappeared under an antiphlogistic regimen, rest in the horizontal position, and mild local applications, as speedily as I had ever seen them disappear in similar instances in which mercury was employed." (p. 87.) Loose and general corollaries, such as these, are only calculated to plunge young practitioners into unprofitable debates in the support of an inert and tedious practice, unless the ways and means of cure are illustrated more fully by a detail of symptoms and cases. There is, perhaps, no use in my remarking, that Dr Thomson has begun at the wrong end of the history; for he speaks first of the constitutional, and then of the primary symptoms, without stating the character of either.

If I did not consider the weight which Dr Thomson's authority might have with some of the junior members of the profession, and the consequent mischief arising from a dissemination of his doctrines, I should be one of the last of persons to oppose his conclusions; and, indeed, I come forward now reluctantly, but principally to vindicate the practices which I have myself successfully followed.

Another part of the same memoir contains the following concise, but sweeping sentence: "The results I have now stated to you are satisfactory, I conceive, in so far as they seem to establish the possibility of every symptom of syphilis being cured without the use of mercury, and by this, to lead to applications of the utmost importance in practice." (p. 90.)

As I consider, that the whole of Dr Thomson's memoir, already referred to, will be perused by the readers of this, I shall make no further extracts, but proceed to contrast the inferences drawn from it, with those of our best practical writers; also, with cases resulting from my own practice, in the hopes of deciding on the most safe and certain way to cure syphilis, as well as to illustrate the good or bad effects deriveable from mercury.

I have next to notice, that Dr Thomson has not thought fit to define what syphilis is, nor whether he adopts or rejects the accurate diagnostic signs of that disease, as laid down by Mr Hunter. The diagnosis, therefore, of Mr Hunter is omitted, his observations laid aside, and his doctrines controverted.

However, I would so far agree with Dr Thomson, as to adhere to no authority, however high, provided that it inculcated precepts inconsistent with the truth, and the result of one's own personal observations; but, I must remark, that there does exist such accuracy of description, and such a cautious recommendation of remedies in the whole of Mr Hunter's book on the venereal disease, that I cannot witness any attempts to overthrow such a stable edifice of facts and observations, without questioning the grounds upon which such attacks are made.

When we consider, that before the time of Sydenham, there existed no systematic account of gout or small-pox, so, before Mr Hunter wrote, little that was certain or clear had appeared respecting venereal complaints, we must allow, that Mr Hunter did more than any other man towards their history and elucidation. But no one would hence assert, that Mr Hunter's book is faultless, though most people will grant its general merit. Not only the diagnosis, but the treatment of syphilis, as inculcated by Mr Hunter, is negatively discarded by Professor Thomson.

Of the accurate description of chancre, as laid down by Mr Hunter, (p. 219, Hunter by Adams,) no notice is taken by Dr Thomson, who would seem ignorant of the description, and leave his readers to infer that even this can be cured without a particle of mercury. I must in justice to each allow, that such a sore upon the genitals has very rarely occurred in my own practice, but still I doubt not that it will sometimes appear. Syphilis then, I define to be that sore, which I shall presently detail, and pseudo-syphilis, a numerous class of anomalous diseases, often resembling the former, but requiring very different treatment. It is matter of surprise, that any one should write upon the two diseases, and not consider the above distinctions of any avail, but call every sore " syphilitic," which arises from sexual intercourse, whether primary or secondary. Mr Hunter describes chancre to be this: " An itching with slight inflammation is first perceived, a small pimple then appears full of matter, with very little tumefaction, and without much redness. If on the prepuce more inflammation follows than on the glans, the itching changes to pain, the pimple or small abscess to an ulcer. A thickening of the part comes on, which at first, and while of the true venereal kind, is very circumscribed, *not* diffusing itself gradually and imperceptibly into the surrounding parts, but terminating rather abruptly. Its base is hard and its edges a little prominent. Venereal ulcers commonly have one character which, however, is not peculiar to them, for many sores which have no disposition to heal, (which is the case with a chancre,) have so far the same character. A chancre has commonly a thickened edge and base, and although in some the common inflammation spreads farther, yet the specific is confined to the base." Such is Mr Hunter's very accurate description of a chancre, and, if it is possible for description alone to convey just ideas, surgeons cannot easily mistake it. Mr Hunter, too, drew a distinction between " specific" and " common" inflammation, but, alas! his distinctions are omitted " en masse."

Dr Thomson has not shewn, that the primary sores, which he has treated without mercury, were of the nature of those above detailed; and until he has given a series of such cases, cured by sarsaparilla exclusively, we must be permitted to question the alleged virtues of this root, to the extent stated by the Professor. Although the occurrence of such a sore is unfrequent, yet I have found mercury to prove its surest antidote. I have never placed implicit reliance on the " visus ineditus" of older surgeons, but yet, I have found the diagnostic signs of Mr Hunter generally useful in practice, by shewing the great frequency of pseudo-syphilitic diseases, compared to those really syphilitic.

That the practice of using mercury, for the cure of such diseases, has been most pernicious, extravagant, and undiscriminating, I have never doubted; but often had reason to deplore the wretched condition of objects placed under my own care, with ruined constitutions, noses gone, palates corroded, and bones carious; all so rendered by mercury unnecessarily employed.

The remarks, therefore, of Dr Thomson, by going to the greatest possible extreme on the one hand, may be productive of ultimate good on the other, though, as yet, they have only unsettled the minds of practitioners, and proved the instability of medical science; and the notice which I shall now take of them, may also lead to deeper investigations, and to fuller reports. We are, therefore, to begin "de novo" with the treatment of syphilis, and to receive all innovations with a degree of caution and scepticism.

Granting, for a moment, that every possible variety of disease, termed venereal, can be cured without mercury, we ask what is to be said of the experiments made by Mr Pearson on certain articles of the materia medica, at the Lock Hospital in London? Are they to be looked upon as futile and nugatory? Are the inferences deducible from them to be considered as ill founded and useless? As an individual, I have been always accustomed to attach great credit to the works of Mr Pearson, thinking them the offspring of much patient research, and accurate observation. Yet this work, the value of which is generally acknowledged, and that of the unparalleled Hunter, may be both committed to the flames, as worse than bad, if Dr Thomson can prove the inutility and inefficacy of mercury in all cases of syphilis. Mr Pearson has clearly shewn, at least in my opinion, that neither guaiacum, sarsaparilla, cinchona, nor cicuta, nor either of the acids, can cure syphilis, though they will remove many diseases resembling it. Dr Thomson, therefore, has no trifling task to perform in combating authorities and facts, high and *mighty* as these are; and he is bound to set forth the whole result of his experience, if he still means to annul every thing that has been written or done by his ancestors.

It is not by lax, vague, and general reports made to the public through the medium of a journal, that the great channels of medical practice are to be turned from their course, and rules of the highest importance unequivocally laid down; but by assembling a large body of cases carefully and impartially detailed, and then by deducing from them such inferences as must be at once natural and inevitable. Before we can repose proper belief in the reports of Dr Thomson, it must be shewn, that the cases wore the syphilitic character; for, if they wanted that character, we may then believe that they were cured by sarsaparilla, or by any thing else, in short, which tended to invigorate the functions of health. And if no cases are to be found in Dr Thomson's list, clearly syphilitic, then we must inquire what is become of that disease? Did Mr Hunter counterfeit all the signs of a chancre? and did he enforce mercurial treatment from prejudice or misconception? I consider, that all these questions are to be answered by any one who comes forward with a neglect of all definitions, and tells the medical world, Gentlemen, ye are all wrong, there no longer exists any necessity for mercury in venereal complaints; sarsaparilla will cure them all.

If syphilis does exist in the present æra, as it did in the days of Mr Hunter, must we not infer that his practice was wrong, if it can now be cured with greater safety to the patient? and if no primary or secondary ulcer can be found to possess those characters which Mr Hunter gave of a true chancre, then we may reasonably conjecture that the disease, to a certain extent, is banished. It certainly is an inquiry very interesting to ascertain the comparative frequency of syphilis, as it now occurs with other diseases. When Mr Hunter wrote about genuine syphilis, he was also aware that a great many diseases, caught from a similar source, resembled it, as his writings sufficiently prove.

Even so old a writer as Celsus remarked, that there were not less than eight different kinds of sores, which affected the genitals; and modern experience would seem to confirm the occurrence of a number far greater. The mere friction of some parts upon others will cause blisters and excoriations, and if urine or morbid secretions be applied to them, rhagades, or spreading and sloughy ulcers, will be the consequence. These seldom or never require mercury. Enough of such facts are upon record, but they wear nothing characteristic of syphilis about them. If we wanted more, comparative pathology, which it is hoped will be some day more extensively cultivated, would furnish them. Farriers have frequently observed that bulls, which have been admitted to a great number of cows, become occasionally affected with ulcers of their penis and sheath; and that those ulcerations can be made to heal by casting the animal, and applying lotions of an astringent nature. No one ever thought of mercury being necessary for the cure of such complaints, nor heard that bulls were ever poxed. Secondary symptoms do not, therefore, necessarily arise from such ulcerations. When a male and female come together, they do not always wait for the increased secretions which generally precede coition, and they therefore excoriate each other, and produce a breach of surface very favourable for the seat of a spurious disease, and for the absorption of it producing constitutional symptoms of some kind.

Rams, which have been known to cover from 80 to 100 ewes of a night, have never been observed to be affected with ulcerations of their penis or sheath, at least not by me. Friction, therefore, must be regarded as a cause differing from that of a specific virus, which I conceive to be the origin of syphilis by inoculation These considerations I adduce, merely to unfold some of the more probable sources of diseases which occur in the same way as syphilis does, but which differ from it; syphilis being considered a disease as much specific as either small-pox or measles. Considering how numerous and multiform these anomalous diseases are, I cannot but think, that an inquiry, instituted with the express purpose of determining their proportion to every single case of real syphilis, would be both highly interesting to surgeons, and important to mankind in general. Now, I am willing to pay every possible deference to the reports of Dr Thomson and the other military medical gentlemen mentioned in his memoir; for they, at least, shew in some degree the comparative frequency of the anomalous with the regular disease, and demonstrate the possibility of curing pseudo-syphilis without mercury. The question, then, which I ask, and it is one which demands a candid answer, is this: "Since the commencement of the non-mercurial system, has there occurred no case of primary sore which resisted a cure from sarsaparilla, and required mercury for its correction?" If it can be said that no ulcer has been seen out of many hundreds taken in regular succession, and not selected, to require mercury for its cure, although the ulcer bore the syphilitic character, then I will admit that the Hunterian diagnosis is useless, and that mercury may be for ever and anon abandoned. But what I have seen, I cannot doubt. I have observed a true chancre get from bad to worse; a bubo has formed, and been followed by an ulcer in the throat, copperish-coloured blotches on the skin, ophthalmia iridis, and so forth, and the whole been cured by an adequate course of mercury. The fact then is, that syphilis is not curable by any thing so soon as by mercury, at least not in this country, according to the concurring testimony of our most experienced and best observing surgeons. The results given by Dr Thomson will be likely to mislead, for the trials have not been continued long enough. Cases have occurred, and will again appear, curable by nothing so speedily as by mercury. I grant that the negroes in the West Indies, to whom mercury is almost unknown, have the credit of curing venereal patients with vegetable preparations; and the Portuguese also for a similar power; hence the Lisbon diet drink, (as it is called;) but we have no well authenticated accounts that the genuine syphilis of Great Britain bears a direct resemblance to the diseases so cured in Portugal and the West Indies.

Syphilis is an ulcerative disease, and gets gradually from bad to worse; whereas the mercurial is characterized by sloughing.

Amidst the jarring and conflicting opinions of modern authors, what course is the student or young surgeon to follow with a case of syphilis when it comes under his care? In one part of the kingdom he might have learnt to salivate patients nine times over; in another he will be told to give mercury in mild and unirritating doses; and, in a third, he will be surprised to hear the discovery that there is no necessity at all for mercury, but that sarsaparilla is the remedy for all diseases of the venereal kind. How to reconcile these opposite and contradictory opinions one cannot at first tell; for an inquiring mind must naturally wonder whether the difference can actually arise out of diseases themselves, which must be supposed to have something of regularity in their course at least, though they may be modified by different constitutions; or whether the differences spring from fanciful and creative imaginations. Practice, therefore, alone can make a good surgeon, since the best authorities are liable to be assailed and rendered questionable.

Although I believe that there is no laying down decided rules of practice in books for the guidance of men young in their profession, yet I have to lament, in common with many, that the conclusions to which experimentalists come are oftentimes so contradictory.

By avoiding Scylla surely one need not fall into Charybdis; by doubting the necessity of mercury in some cases, one would not withhold its use in all; nor, by having observed its good effects in many, would one carry it to an extreme in every instance. It is to the general conclusion to which I principally object, viz. that no case of syphilis can require mercury; or, in other words, that every case will disappear as speedily without as with mercury. *

Other surgeons have made similar trials with this root, and declared that it hath no power of curing syphilis; † but whether or not Dr Thomson has discovered a more efficacious sort than has been hitherto found, he has not stated. If this fact can be established, I grant that a most important piece of know-

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* Edinburgh Journal, No. 53.

† Pearson's Surgery.

ledge is at length gained: but can we suppose that all surgeons, from Hunter downwards, have been deceived, never observed for themselves, nor made experiments to determine a point so essential to pathology? Can the whole mass of medical evidence recorded for ages past, and now hoarded amidst the most precious arcana of medical literature, be quashed by one brief and solitary memoir from the pen of Professor Thomson? Surely not. Mercury is still useful.

In the medical reports of Dr Beddoes for the West of England, some practitioners took it into their heads to think as well of nitric acid as others now do of sarsaparilla. Pamphlets appeared upon its virtues, and mercury was by them laid aside for a time, but its use resumed very soon when nitric acid was found to be totally inadequate to the cure of syphilis. Such is the way in which some practitioners fluctuate in opinion, and shew that they have no settled notions about diseases or their remedies.

The experiments which have been instituted in the different regimental hospitals throughout North Britain by Mr Hennen, deputy-inspector for the district, shew most certainly, that the majority of anomalous diseases can be cured without mercury; but we have not been favoured with sufficient details to offer any reasoning upon the cases. My best acknowledgments are due to Mr Hennen for the liberal, candid, and gentlemanlike manner, in which he directed that I might inspect the different regimental hospitals in Edinburgh during my stay here, and take under my immediate care any cases which I deemed interesting; and my thanks are likewise due to Surgeon Johnston of the 88th regiment, for the impartiality and candour with which he has imparted to me the result of his own experience upon trials made in the hospital under his charge. However strange it may seem, but I have seen no one instance of either primary or secondary affection which could be characterized as syphilitic, according to the Hunterian distinction, during my visits at the regimental hospitals in Edinburgh.

The experiments also made in our different military hospitals throughout England, and some parts of France and Spain, already made known to the public by several surgeons, have conferred inestimable services upon mankind at large; by shewing how unnecessarily mercury has been heretofore resorted to for the cure of venereal cases, to which it proved a bane rather than an antidote. It is to be hoped that the director-general, who has already done so much good for the army medical department and science at large, will see the necessity of making himself a centre or channel of communication for all reports made upon this much agitated and discordant subject, that the public may be favoured with the accumulated experience of army-surgeons, and deduce from them leading principles of practice.

Then will the comparative relation which irregular diseases bear to genuine syphilis be determined; and then also shall we learn if mercury deserves still to hold its place in the cure of venereal complaints, or to be consigned to eternal oblivion. It is a matter of indifference to me which way the decision is made: I desire only to ascertain what can be proved as to the existence and cure of syphilis. For, if established practices and received notions can be thus upset every whip's while, we must be inclined to doubt all old deductions from practice, and to infer, that surgeons have in succession followed the authorities of their predecessors, and not attended to the actual operations of nature, as they ought to have done.

Our army hospitals have therefore done great good already by unsettling the deeply rooted opinions of many practitioners, and disposing them to unlearn a great deal of what they had taken so much pains to acquire. But at present the cure of no one class of diseases is more indetermined than those called venereal. In my opinion, therefore, the first part of the investigation should be, to shew all the errors of the old practice; and then to set forth, clearly and satisfactorily, the advantages of the new. It is no uncommon question to be asked, whether one is a mercurialist or an anti-mercurialist, just as a member of the House of Commons is interrogated as to the side he takes; as if the establishment of the surest and best treatment for the good of mankind has any thing to do with party business. Two men may see the same disease together, watch its progress, and record the effect of medicines upon it, and yet their opinions may differ, not from any wish to oppose, but because they do not conscientiously think alike. As long as this imperfection does exist in human nature, all theories and all aphorisms must seem questionable when logically examined, because medical is unlike physical science, and cannot be reduced to mathematical demonstrations, like the laws of gravity, and so forth. I have no other reason for differing with Professor Thomson in opinion, but because I think differently from him. Pathology is a science of observation. We might have both observed facts corresponding in their nature, and yet make opposite conclusions. Dr Thomson asserts that syphilis can be cured, " in every instance," without mercury. On the contrary, I should say that there are diseases which nothing but mercury will cure. The Professor abominates mercury, whilst I hold it in great esteem.

I beg here distinctly to state, that it is upon public grounds exclusively that I differ with Dr Thomson, and with feelings the most respectful, for I am well assured that he is a man distinguished for originality of mind and amiability of character.

In looking to the cure of any disease whatever, two leading objects present themselves for consideration; the first is safety,—the second celerity. People in general do not want to be told whether or not a disease is syphilitic. If they have a venereal sore, they know how it was caught, and want only to have it cured; but nosological distinctions should exist amongst surgeons. Let any patient have sores upon his penis, and find them get worse and worse, no surgeon, however eminent, will prevent such a one from flying to mercury, or at least to something from which he expects a cure. In following Dr Thomson's rules, I presume that mercury is to be laid aside in primary ulcers, and sarsaparilla substituted.

If, therefore, a sore like a chancre has been observed upon the penis, for instance, and it goes on to the formation of a bubo, in the regular course of syphilis, during the administration of sarsaparilla, would any unbiassed surgeon hesitate to begin mercury? Or, supposing that it had been thought fit to withhold mercury for a time, and a syphilitic ulcer appeared in the throat, could it then be justifiable to refuse a use of mercury? Common sense, upon which all medical science is founded, as well as upon observation, will answer such questions. When a chancre appears upon the penis, or any other part, it ought to be cured as soon as possible, as I conceive, with safety to the constitution; not locally, but through the medium of the constitution. Mercury not only causes primary sores to heal, but prevents the contamination of the system, which sometimes arises from sores not syphilitic.

No one, then, in my opinion, can be justified in hazarding the possibility of secondary symptoms appearing, by abstaining from mercury, which generally prevents them; since it has never been proved that sarsaparilla can fortify the constitution against these attacks, though it may cause some local sores to heal.

In chronic ulcers of the leg, it is well known that a few doses of the common Cinchona bark will give them a healthy and granulating aspect; and so it may do in indolent ulcers upon the penis; but if those last possess any thing specific in their nature, the chances are, that absorption will take place, and constitutional symptoms arise, where mercury has been withheld. I speak from what I have seen in such cases.

There are some sores to which mercury will prove manifestly injurious. In those which have a tendency to slough, or to burrow rapidly, mercury will generally accelerate these processes, at least with as much certainty as purging and rest is found to check them.

The work of Mr Matthias on the mercurial disease merits high commendation, for shewing what this disease really is, its analogy to syphilis, and the destruction it produced when mercury has been rashly and ignorantly employed.

The elaborate and excellent work of Mr Carmichael of Dublin has also done infinite good, by the clear histories which it contains, and the accurate representations its plates appear to afford, of the diseases for which the descriptions are given. But neither of these authors has denied the beneficial effects produced by mercury in removing some venereal complaints, though it acts as a poison on others; and both admit the possibility of cases occurring, in which it is possible for this mineral to do either much good or much harm.

I was educated in a school, fortunately for me, in which every one was taught to look to nature and actual diseases, as they occurred, rather than to uphold authority. It was in this school, at St Bartholomew's Hospital in London, that the principal distinctions were made between the nature, characters, progress, and cure of diseases really syphilitic, and those which only resemble them. On the reader's referring to the admirable work of Mr Abernethy on diseases resembling syphilis, he will there find a series of practical observations and patient reflections, drawn carefully from facts as they presented themselves, with the modes of cure annexed.

Although Mr Hunter did most undeniably write of anomalous deviations from the venereal, yet the class of pseudo-syphilitic diseases owe their chief elucidation to the vigilance of Mr Abernethy. To that distinguished professor the medical world is peculiarly indebted, not merely for his luminous and scientific illustrations of the diseases in question, but also for some of the soundest pathological researches upon other subjects ever made by men. His experience has satisfactorily shewn, that genuine syphilis is rare indeed; nay that, in the present day, it is seldom seen; but that where it does appear, mercury is the most certain cure for it. Respecting the phenomena of pseudo-syphilitic diseases, the observations of Mr Abernethy were at one time so peculiarly and entirely his own, that he even took the pains of calling upon many of the most experienced and eminent surgeons in the metropolis, to ask if they had ever observed syphilis to get well spontaneously. All replied in the negative, but universally agreed that it got worse if no mercury was used.—(Surgical Observations.)

We also learn from the same wise author, that tribes of diseases resembling syphilis occur everywhere, and that such admit of different methods of cure. Some require pretty full courses of mercury; some only mild, unirritating, and undebilitating doses; whilst others will bear scarcely any mercury at all. These differences are to be sought for only in the varying nature of diseases themselves, as modified by various constitutions. In confirmation of the doctrines which I am now advocating, I may remark, that in the course of eight years' regimental practice, I have not had one case of venereal disease which has done otherwise than well; I mean, that every man has gone to his duty again soon, when the treatment rested originally with myself. I have seldom found it necessary to salivate patients, but that the plurality of the cases entrusted to my care got well under mild but efficient doses of mercury. In many primary sores I have given no mercury at all, except in purges. I repeat, that eight years have elapsed without my having had one incurable case of syphilis, where the primary sores were treated by myself; but I do not assume credit to myself for this success, but to that useful school and excellent preceptor from whence I first learnt the principles of medical surgery. I must, however, allow, that, owing to the morality so laudably inculcated by the officers who have commanded the South Devon Militia since my appointment to it, as surgeon, the field for my observing and treating venereal diseases has been comparatively small. The greater number of the cases by which the preceding axioms have been confirmed, occurred in my private practice, unconnected with that of the regiment.

No rules can be laid down for the treatment of venereal diseases, as I believe, which rules cannot be liable to occasional deviations. Some sores will be fast healing under the influence of mercury, then get to a stand, and spread rapidly. Others will heal in an uninterrupted manner. A certain quantity of mercury, which strengthens and invigorates the functions of health, will frequently cure diseases which had resisted a more powerful course.

On the first sight of a patient, how can any surgeon pronounce whether or not he will bear mercury? It is, therefore, no bad practical rule to watch the progress of a sore whose nature cannot at first be determined; and if it gets gradually worse, to notice the effect of mercury on it. Mercury can never be the antidote for any disease if it is made worse by it. The intention of every remedy must be to make a patient ultimately better. But precaution is very necessary in employing such a potent antidote as mercury. A great degree of uncertainty sometimes attends its action, as well as that of other medicines. For example, ipecacuanha will not always vomit in an ordinary dose, nor jalap purge by the same rule, nor opium invariably produce sleep,—so mercury cannot be made to salivate some persons, though their constitutions be charged with it. This degree of uncertainty attending its operation has been one great cause for the dissention from the mercurial plan of treatment; but all practitioners must calculate upon uncertainty, more or less, in the operation of all pharmaceutical preparations. In this polemical age, it seems to be the fashion to distrust and unlearn many opinions which were formerly deemed incontrovertible; but one ought to be cautious in forsaking the good till better are advanced.

Since it is asserted that all kinds of syphilitic sores, ulcers, nodes, &c. disappear as readily without as with mercury, * let us ask the whole medical profession if such appearances are not often cured by mercury after the failure of sarsaparilla and other medicines? The fact is, that Dr Thomson goes too far on the one hand, as older surgeons have done on the other. Success will depend on neither; but, like beauty, be found to consist in mediocrity. The truth is seldom found in extremes, but in the happy medium which neither abstains from mercury " in every instance," nor pushes it to nine successive salivations. It is by steering clear of extremes that we shall be most likely to hit upon a right mode of treatment. Mercury is still useful, and although its powers are great, its value is nevertheless undiminished. A longer perseverance in the non-mercurial system will, I have no doubt, point out the increased frequency of constitutional symptoms arising from local sores, and lead its present opponents hereafter to acknowledge it as one of the most valuable medicines known, both in the cure of syphilitic and pseudo-syphilitic diseases.

Mr Hunter again says, " when the venereal action is once set up in a part, the constitution has no power of altering it." † How this clause can be got over by those who recommend sarsaparilla and substances equally inefficacious, I am at a loss to determine. Still I think it desirable to ascertain how far modern experience substantiates Mr Hunter's idea. When it was first observed that mercury could cure syphilis, or that disease which it is said first broke out at the siege of Naples, the ancients thought it also capable of curing every form of spurious disease

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* Vide Edinburgh Medical and Surgical Journal, No. 53, p. 85.

† Hunter by Adams, p. 67 and 135.

which followed sexual intercourse; but such is not the fact. When I was at Naples in July 1817, I saw a great number of sloughy venereal sores in the " Spedale degli incurabili," which were treated by an antiphlogistic regimen chiefly. The disease called " the Black Lion" often spreads like pestilence amongst the Lazaroni, who cohabit almost as promiscuously as animals, and live in filth and nakedness in the open streets of the city

In the hospitals at Paris, where venereal diseases are not, according to the population, so common as in London, owing to the public baths, mercury is not used near so extravagantly.

The extreme of prohibiting mercury in a real case of syphilis is just as insecure as the practice of repeated salivations is cruel and destructive in the management of pseudo-syphilitic diseases. In many spurious diseases when mercury is first used, they often get better; mercury is increased, and they get worse. Does not this argue that, to a certain extent, it proves beneficial, but injurious if carried beyond a certain quantity? How it acts is another matter; for most people, on commencing the use of the pilula hydrargyri for instance, find themselves invigorated, but when the mercurial action begins in the constitution they lose as fast as they had before gained flesh.

I have already remarked that Dr Thomson has not thought fit to pay attention to any nosological classification. Every eruption and ulcer is with him syphilitic; and every syphilitic symptom can be cured without mercury. This is the sum total of his memoir. And the Professor further calculates, that these doctrines will " lead to applications of the utmost importance in practice." * I sincerely wish that they may do so, but, with a mind fully open to conviction, I shall never be satisfied until he has adduced proofs more positive, and arguments more solid. At least I will evince a desire to hear the result of his additional inquiries upon the subject; hoping that these comments, which are meant to be void of all personalities, will afford the Doctor another opportunity of coming forward to corroborate his statements in a more ample and explicit manner.

Before I close the present communication, I think it but just to notice the important facts lately adduced by Dr Farre in proof of the " modus operandi" of mercury; of the particular state in which it is beneficially applicable, and of others in which it proves detrimental.

The late Mr Saunders, of whom this nation ought to feel

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* Edinburgh Medical and Surgical Journal, No. 53, p. 30.

very proud, first described the importance of mercury in arresting syphilitic iritis, which led to blindness almost invariably when mercury was not employed as a specific. Mr Saunders, unfortunately for mankind, did not live long enough to give to the public that extensive benefit of which his discoveries were capable. It was, therefore, reserved for the accurate observation and patient investigation of Dr Farre to demonstrate to mankind the value of those stores, which, though long known to a certain extent, had not been reduced to that order and certainty of which he has now rendered them applicable. Dr Farre has explained the action of mercury not only in arresting syphilitic iritis, but also in stopping iritic inflammation of an idiopathic form. Dr Farre reasons philosophically, and says, "The certainty with which the mercurial action arrested the deposition of coagulable lymph in syphilitic inflammation of the iris, led the editor (Dr Farre) to give this remedy a fair trial in simple inflammation of the iris, in which the disorganizing process by the adhesive inflammation is precisely the same, however it may differ from the former in its exciting cause. The result of the trial has perfectly satisfied him that the mercurial action *alone*, when properly kept up, is sufficient to subdue the ophthalmia iridis in its most acute stage."* The action of no remedy was ever more satisfactorily cleared up than this, at least in my estimation. Mercury then acts by preventing an effusion of lymph in the adhesive inflammation, whether it be idiopathic or syphilitic. Mercurial preparations have been so generally employed, because they were observed to cure numerous complaints, but no one has ever described the precise state of morbid actions likely to be rectified by them, before Dr Farre. His remarks, therefore, are second to none; his pathological reasonings are so sound, and his doctrines fraught with such temperate and excellent judgment, that I do conceive both the profession and the public stand greatly indebted to his valuable labours, by which this mysterious and long sought for problem has been at length reduced to its elements, viz. the *modus operandi* of mercury.

Now, will it be found that the adhesive inflammation leading to an effusion of lymph, and the syphilitic running to ulceration, will be both checked by one and the same remedy which frustrates disorganization? Whilst I can, from experience, vouch for the real value of this singularly important information, I ought to express my public respect towards a man so deservedly high in his profession, and confess my obligations to him for personal favours, not long since received.

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* Saunders on the Eye, Preface to Second Edition, p. 33.

In his first fasciculus upon disorders of the liver, Dr Farre has denoted the peculiar diathesis which is unfavourable for the action of mercury; and he has now advanced proofs of the morbid states which it will correct, proving thereby the good or harm to be done by mercury. These are principles to go upon; and it is unhappy that they should meet with any alloy. Mr Travers, in his Surgical Essays,* has, however, shewn, that mercurial actions, when seated in the iris, are nearly allied to the syphilitic in appearance, and that mercury rather favours the continuance of them than otherwise.

The near resemblance of the mercurial and syphilitic actions have doubtless created such vast confusion in practice, rendered practitioners sceptical, and brought the mineral into disrepute. Under such circumstances, it is very probable that older surgeons, finding a venereal affection got worse, pushed mercury to a pernicious extent, and did more injury by the antidote than the disease itself would have produced. And it is not less improbable that the numerous piles of skulls and bones, contained in the catacombs at Paris, afford examples of the great destruction done by mercury, though they were described to me as specimens of the venereal.

In discussing any subject for the good of humanity, it is right to give every measure a fair and impartial consideration. By confessing the immense devastation which mercury can do, one would not abolish it *in toto.* The pathological researches made upon the eye, as the index of general inflammation, its progress and termination must tend to the cure of diseases with greater certainty; to increase the confidence of the profession in its own powers, and to enhance the value of one of the most useful institutions in Great Britain; conferring thereby additional lustre on the names of its distinguished founder, the late J. Cunningham Saunders, Esq. and present supporters. The useful observations which have been made at this institution I view in no ordinary light, for they not only shew how and when mercury is necessary, but when also it will be beneficial or injurious.

Mercury, therefore, will still retain its credit as a specific against syphilis, and rank not only as a febrifuge, but next to bloodletting, as the most to be depended upon of all antiphlogistic measures.

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* By A. Cooper, Esq. and B. Travers, Esq.

Its judicious application to arrest the progress of iritic inflammation, will also lead to its use in the first stage of disorganization in all the important vessels of the human body. The inflammatory process being set up in the heart, the lungs, the liver, &c. may be subdued with equal certainty as in the iris; if we can trust in the least degree to analogy, and I have no doubt that we can do so. The fault, therefore, lies not in the absolute rejection of mercury, but in the unskilful use of it; in taking up principles slightly from superficial observations made on a few facts, unwarrantably generalized. In taking my leave of this subject, I cannot too strenuously recommend the admirable work of Mr Abernethy, which, with me, is quite the standard for the management of mercury in syphilis; its precepts being confirmed by the evidence of facts, and test of experience.

January 22, 1819.

VI.

Case of Hydrophobia. By JAMES JOHNSON, M. D. one of the Physicians of the Dundee Dispensary.

> When burning fever furs the parched tongue,
> Water its heat and dryness can assuage;
> When febrile ardour dries the tender skin,
> The gelid fluid quickly brings relief:
> But me, alas! whose tongue rolls hard and dry,
> Whose thirst is greater than can be endured,
> From this refreshing liquid am debarred;
> At mention of its name I'm tossed and pained.

ON the 25th day of November 1818, George Hume, weaver, Perth Road, called at my residence, with his daughter, a girl about nine years of age, who had been bitten on the cheek by a dog, supposed to be mad. Being from home, the man was advised to make immediate application to another practitioner, and not await my return, which it was known might not be very soon. On my return, I was informed that the above patient had called upon me, and, not knowing that any farther application had been made, and there being something particularly interesting about the case, I called to see what had been done. A slip of adhesive plaster was on the cheek, which being removed, the wounds appeared, and they were very slight,—little more than through the cuticle, but in a state of suppuration: their form and relative situation satisfied me that they had been inflicted by the teeth, and not by the paw of an animal.

The character of the dog was next inquired into. He was a small, useless, cowardly, domestic cur; had lived about eighteen months, and was never known, till now, to have bitten any animal to the effusion of its blood. He had access to carrion, and was known to have availed himself of it. He became listless, and unusually sleepy. His eyes were suffused, and were compared to the eyes of a person at the commencement of fever. When food was presented to him he retired to a solitary place, below a chair, or a bed, and snarled when attempts were made to bring him out. He attacked cats, with which he had been domesticated; dogs which previously he avoided; and persons whom he formerly respected. He snapt at several dogs and persons while walking on the road near his abode, although they gave no provocation. With one dog, which he formerly shunned, he fought a considerable time, and continued fighting until separated. He made his attacks without any noise, and desisted when he had given the snap. He shewed no aversion to water, and took some of it a little before he was killed. Such deviations from his usual habits satisfied me that he must have been mad.

Seventy hours had elapsed since the child was bitten. There was more pain and swelling in the wound and the side of the face than could have been expected from a common wound of much larger dimensions. After explaining the matter to the parents, it was proposed to cut out the bites, to which they agreed, and the excision was immediately performed. The recent wound made by the knife was constantly washed for a full hour with tepid water, and an emollient poultice applied over it. Next day it was carefully bathed with oxymuriatic acid, according to a suggestion published in your Journal, from M. Orfila's book on Poisons. The edges of the wound were brought together with adhesive straps, in order to make the cicatrix as neat as possible. In two days a complete adhesion of the wound was effected. The pain and swelling abated, and soon ceased. The cicatrix was destroyed with lunar caustic, and the eschar allowed to fall off spontaneously. The child looked healthy, and was playful. She took laxatives, and thirty drops of oxymuriatic acid in water, three times a-day. Two ounces of the liquid acid were taken internally in about thirty days. A dog, bitten by the same animal which bit this girl, became evidently mad in two days after being bitten, which satisfied me that I had done right in removing the bites.

About the third week after the accident, the little patient was observed to have disturbed sleep, and she often awoke in a fright, which excited the apprehensions of her medical attendant. On Saturday, 2d January 1819, on being offered some tea, the characteristic signs of hydrophobia manifested themselves. She wished for drink; but, when she put the cup to her mouth, started from it, as a child is observed to do when something too hot is presented to it,—made a deep sigh,—appeared astonished, and angry with herself that she could not satisfy her thirst. She again took the cup in a determined manner, and, after some thoughtful hesitation, brought it quickly to her lips; but it was suddenly repelled, exactly like a light body coming in contact with an electrical machine. The pulse was about 100. Much pain was felt in the wound, the side of the face, and at the pit of the stomach,—what she called her breast. The heart palpitated. A cold hand applied to the region of the heart induced a deep sigh, and increased the pain at the stomach. No pain was felt in the throat. She was quite collected, and suffered little pain, except when the sighs were brought upon her. During the day she took a little food and drink, in very minute portions, but had much difficulty in swallowing. Attempts were made to bring her system under the influence of mercury, and rubbing with the strong mercurial ointment was assiduously employed. The throat and breast were rubbed with anodyne balsam. She passed a restless night, without any sleep, and on Sabbath morning the symptoms had evidently increased. The mercurial frictions brought on the sighs, which were now attended with evident spasms of the arms and legs. The rubbing was blamed for stiffening and taking the power from the arms and legs, and it was found impracticable to continue it longer. An attempt was made in the evening to administer an injection of laudanum. The application of the pipe to the extremity of the rectum brought on most severe spasms, and the outlet of the bowels was so constricted, that it seemed as impracticable to introduce the pipe into the rectum, as it would have been to push it through the parietes of the abdomen. This attempt gave the girl an antipathy at all the doctors, and she requested her father frequently during the night to go and inform them not to come back. She still continued to swallow a little sweet wine. She could not look upon a quantity of any fluid; a glassful or a cupful distressed her very much; even a tea-spoonful did so; yet she could look upon half a tea-spoonful. Fluids were conveyed from a tea-spoon to her mouth with her finger. A transparent drop at the finger, or at the bottom of a spoon, was disagreeable to her. She frequently refreshed herself by sucking water and wine from a soft rag, previously dipt in these fluids; but deglutition always brought on pain. When she seemed to be taking drink without pain, it was found that she was collecting it in her mouth, and not swallowing. Only a very small quantity seemed to get down at each effort of deglutition; for the swallowing of half a tea-spoonful seemed to require a succession of efforts. The motion in the neck and shoulders, when swallowing was attempted, resembled that which may be observed in a young raven, when fed by its dam. Perhaps the succession of efforts alluded to may be explained by supposing that the fluid got over with the first, but that the muscles, when once thrown into action, involuntarily repeated that action several times before they settled in their former state. Sabbath night was also passed in a very restless manner, and on Monday the power of swallowing was completely lost, and the dread of fluids, or of any thing which brought them to the mind by association, was excessive. The presence of strangers was very disagreeable. She requested to be left with her friends. She was frightfully agitated at the first sight of the medical attendants on Monday morning. If they had not speedily retired, it appeared that she would have struggled till she died. During the forenoon, a complete reconciliation took place between her and the physicians; but the presence of more than one or two strangers was intolerable. A peculiar, rolling, jealous wildness of the eyes was remarkable from the very commencement. She wished to look in all directions at once, and seemed as if haunted. The medical attendants were now obliged to stand inactive spectators of the shocking scene. The disease advanced without any thing remarkable, except a progressive aggravation of the symptoms. Some of the severest paroxysms were preceded by discharges of air from the stomach, which, perhaps, were occasioned by the commencement of the spasms in the diaphragm. Slight uneasiness was felt in the throat, and a gurgling noise, exactly like what happens before death in fevers, was constantly present for the last twelve hours. At one o'clock on Tuesday morning, she desired to be taken out to the air, which was done, and she expressed herself much relieved. At three o'clock she called for drink,—wine and water was given to her,—she drank it greedily, and without feeling any uneasiness. She rejoiced with her mother at being able to drink, and had herself carried to a neighbouring house to shew that she was much better. She then asked for porridge, a tea-cupful of which was made with milk and oatmeal;—this quantity she took easily. About four o'clock she began to talk incessantly, and attempted to sing, by which she appeared to exhaust herself. She again became very low and sunk. The spasms were frequent and severe; they seemed to degenerate into a constant sprawling unmeaning motion of the arms and legs, which was terminated by two unsuccessful efforts to vomit, with which she ceased to struggle, and instantly expired, without any noise or motion indicative of pain.

The pulse, during the disease, arose to 140, and was feeble. The skin was not hot; but a linen sheet was, for the most part, her only covering. Respiration was performed easily, except during the fits. The urine was natural, and regularly voided; but no discharge could be procured from the bowels; nor did she sleep from the commencement of the complaint. It was suspected that the pupil of the eye was dilated; but light could not be brought so near as to enable us to ascertain the fact. Vision was certainly imperfect for the last twenty-four hours. The internal senses continued entire till within two hours of death; constant talking, and attempts to sing were the first and only marks of incoherence For the last hour there seemed to be a complete suspension of the mental faculties, rather than any deprivation of them. The case was seen by many of the medical gentlemen in this town, and one or other of them were witnesses to almost all that is here narrated, so far as respects the phenomena exhibited by the patient in hydrophobia.

Dundee, 16*th January* 1819.

VII.

An Inquiry into an alleged connection between Trismus Nascentium, and certain diseased appearances in the Umbilicus. By SAMUEL B. LABATT, M. D. Licentiate of the College of Physicians, and Master of the Lying-in-Hospital, Dublin.

On looking over the Dublin Hospital Reports, a paper by A. Colles, M. D. on the Trismus Nascentium, strongly attracted my attention, as well as that of some of the practitioners who had preceded me in the Lying-in hospital. My friend Dr Clarke, who had bestowed a great deal of attention on the subject, and certainly not without success, was among the first to suggest to me the propriety, and indeed absolute necessity, of examining carefully by dissection every infant who should die of the nine day fits, that the novel facts alleged by Mr Colles might not rest on the authority of an individual, however respectable. This suggestion, coinciding most fully with my own sentiments, determined me to examine such number of bodies as should afford conviction to my own mind on this important subject.

Before I enter upon a detail of these dissections, I think it necessary to quote Mr Colles's account of the appearances observed by him in those children he dissected.

Having informed us of the considerations which induced him to seek for the cause of trismus nascentium in the umbilical chord and fossa, he proceeds:

"The skin forming the edges of the umbilical fossa was in some a little more raised than usual. When the borders of this hollow were expanded by introducing a pair of dissecting forceps, we observed the floor of this cavity not flat, but considerably raised in the centre by a knob, or large papilla. Both the central raised part, and the surrounding flat parts of this surface, presented all the characters of those new membranes which are formed by suppurative inflammation. In some few instances, the fundus of this cavity presented evident marks of superficial ulceration, confined to the vicinity of the umbilical vein. A probe readily passed through the substance of this central tubercle, and entered into the umbilical vein; such were the appearances presented by the umbilical fossa externally.

"On cutting into the abdomen, the peritoneum covering the umbilical vein was highly vascular, as if from inflammation. This extended sometimes up to the fissure of the liver, often, however, not for a greater length than one inch above the umbilicus. The peritoneum, in the course of the umbilical arteries, appeared still more inflamed, an appearance which extended often as far as the sides of the bladder. Besides the appearance of the peritoneum along their posterior surface, the cellular substance which covered them, and the urachus anteriorly, was loaded with a yellow watery fluid, even down to the bladder. Leaving the umbilicus untouched, if we cut open the umbilical vein, from the liver to the vicinity of the umbilicus, we found only a few small coagula of blood within its canal; the inner surface of the vein was pale, and free from any marks of inflammation, yet the coats of the vein altogether were very much thickened. The umbilical arteries exhibited evident marks of inflammation. 1*st*. On slitting them up, a thick yellow fluid, resembling coagulable lymph, was found within their coats. 2*dly*, In all instances their coats were much thickened and hardened, even as far as the fundus of the bladder.

"On cutting into the umbilicus itself, from its posterior or peritoneal surface, we found in the centre a space about half an inch long, occupied by a soft yellow substance, which bore a very strong resemblance to coagulable lymph produced by inflammation; it was this which formed the prominence observed in the external view of the fossa. The extent of this middle space varied in different cases, but in every instance the arteries opened into it, or rather were lost upon it.

"In a few instances the umbilical arteries presented this appearance, or rather were converted into this substance, to the distance of a quarter of an inch from the navel. The extremity of the umbilical vein was affected in different degrees, in various instances. In some it presented a pouch or varix, which extended one eighth of an inch below the extremity of the opening of the vein, viz. in a direction towards the bladder. In some, the extremity of the vein presented an appearance of ulceration on its margin, and in all, the edges of the extremity of the veins thickened. In every instance, the ends of all these vessels remained open, their canals were in continuity with the soft substance which occupied the centre of the umbilical space, so that a small probe or bristle passed without opposition from these vessels into this soft substance."

The Doctor concludes his description, by observing, "The state of the umbilicus and its vessels, here described, having been found in all the cases I have inspected, and these appearances not being discoverable in infants of the same age which had died of other diseases; is it not a fair inference, that such is the cause of trismus nascentium?"

Respecting the dissections which I am now about to detail, I think it necessary to premise, that they were performed in my presence by Mr C. Johnston, and Mr Shekleton, my assistants.

1st *Dissection.*—The infant died of locked jaw the eighth day. With the exception of a slight appearance of ulceration, the umbilical fossa presented nothing peculiar. Its edges were *not* raised, nor was its floor. A probe could *not* be introduced from the fossa into the umbilical vein. On cutting into the abdomen, *no* signs of inflammation or vascularity could be discovered in the peritoneum covering the umbilical arteries or veins, nor was there the slight appearance of a *yellow watery fluid* in the cellular substance surrounding them. The vessels themselves, on being slit open, appeared healthy, without any thickening, marks of inflammation, or yellow fluid within their coats. No space occupied by a soft yellow substance could be detected on cutting into the umbilicus, from its internal or peritoneal surface; nor did the extremities of the arteries or veins, towards the navel, exhibit any thing peculiar. They were all pervious from within. Bladder was distended with urine.

2d *Dissection.*—This child died the tenth day of a *bowel complaint.* We dissected it in order to compare the appearances with those remarked in the former subject; and we were struck with the similarity they bore to certain parts of Mr Colles's description. The fossa was raised in the centre, into three papillæ or small elevations, which proved to be the orifices of the umbilical arteries and vein, enlarged and covered with lymph. On cutting into the abdomen, the peritoneum covering these vessels appeared more vascular than usual, and the cellular substance surrounding them contained a yellow watery fluid; their orifices remained open, and the coats were evidently thicker than in the first case.

3d *Dissection.*—This subject was carried off by a diarrhœa on the eighth or ninth day. No memorandum of the dissection was preserved, but I recollect there were some of those appearances which are said to be confined to infants who had died of trismus.

4th *Dissection.*—Here also we neglected to preserve notes, but, though the infant died of locked jaw, the umbilicus exhibited but few of the appearances mentioned by Mr Colles.

5th *Dissection.*—A fine plump boy; funis separated on the fifth day; attacked with trismus on the same evening, and died in sixty hours; skin forming the edge of fossa *not* more raised than natural. The central papilla, or raised part, was connected to the skin forming the superior edge of the fossa, so that a probe could not be passed round it. This papilla was occupied in its centre by the opening of the umbilical vein, which was pervious, but there was *not* any appearance of ulceration round it. At the inferior edge of the fossa and base of the papilla, the openings of the umbilical arteries were seen surrounded with yellow matter, and the surface of this portion of the fossa appeared ulcerated.

On cutting into the abdomen, the peritoneum covering the umbilical vein was *not* more vascular than any other portion of this membrane. The cellular substance which covered its posterior surface, was *not* loaded with yellow watery fluid, nor even any appearance of it. On cutting into the umbilical vein, it was found healthy,—*no* thickening of its coats. Umbilical arteries, on being laid open, presented a *white polished* surface. Their coats were *not* thickened, nor was there any yellow fluid within them, but their extremities, as well as that of the vein, were thickened; all the vessels were pervious.

Cutting into the umbilicus itself, there was nothing to be seen but healthy cellular substance connecting the vessels of the umbilicus together; *no* yellow substance whatever, *no* pouch at the extremity of the vein; it opened in the centre of the papilla and the arteries on the ulcerated surface before mentioned.

6th *Dissection.*—A small female infant. Funis separated on the morning of the fourth day; attacked with trismus the same evening, and died in forty hours. The edges of the fossa were *not* raised. Its whole surface looked *red* and *healthy*. The papilla in the centre was like a healthy granulation. There was *no* entrance to the umbilical vein from its centre. The umbilical arteries opened on either side of this papilla. On cutting into the abdomen, the peritoneum did *not* appear inflamed. There was yellow watery fluid covering and passing along the umbilical arteries. At the junction of the vessels inside, corresponding to the fossa without, and under the peritoneum, there was a thickening of the cellular substance, of a yellowish appearance, and pretty firm consistence. The arteries, though they passed through this substance, did not appear thicker than natural. On laying them open to their extremities, their lining membrane was *white* and *shining*, without any appearance of coagulable lymph.

7th *Dissection.*—Child died of trismus eighth day. Fifty hours ill. Edges of fossa *not* raised; *no* ulcerated appearance in the fossa. Papilla *not* covered with lymph. Vein opened in the centre of the papilla; arteries pervious below it.

On the inner surface of the abdomen the peritoneum was healthy; *no* yellow watery fluid covering the umbilical vessels; *no* thickening of parts corresponding to the fossa without. On cutting into the fossa there was *no* yellow substance. Lining membrane of both arteries and vein was *white* and *polished*. *No* thickening of their extremities, nor appearance of coagulable lymph.

8th *Dissection.*—This infant was attacked with trismus the fourth day, and died in 24 hours. Chord not completely separated; detached from the arteries, which were pervious, but adherent to the papilla. Vein opened into the sloughy chord. That part of the fossa from which the funis was detached was ulcerated.

In the abdomen the peritoneum covering the umbilical arteries and vein was *not* inflamed, nor was there any yellow watery substance observed in the course of these vessels. The parts answering to the fossa behind were thickened, or at least larger than usual; but, on cutting into its substance, there was *no* appearance of yellow matter. Lining membrane of all the vessels was white and shining. *No* thickening of their coats, or any appearance of yellow matter within them.

9th *Dissection.*—This infant died on the fifth day, of an affection of the chest. The chord was separated. The edges of the fossa not elevated, but its surface was covered with yellow matter, and looked ulcerated. The centre was occupied by the papilla, * in which the opening of the vein was easily discovered. The arteries were closed at their extremities. On cutting into the abdomen, the peritoneal surface shewed no appearance of inflammation; no yellow fluid along the arteries. The point corresponding to the fossa without was thickened, but not covered with yellow matter. On cutting into it, however, this substance was evident. The arteries and vein were not thickened, nor did the lining membrane of either shew any appearance of yellow lymph. The extremity of the vein was thickened. Mouths of the arteries closed.

Enough has been said, I should hope, to prove that the appearances mentioned by Mr Colles are not always present in infants who have died of trismus; and as they are sometimes found in infants who have died of other diseases, may I not conclude that he has been guilty of drawing general conclusions from partial and insufficient data? With feelings of the highest respect for his professional talents, I venture to suggest to him the necessity of again recurring to anatomical research. That reconsideration of the subject which I recommend to Mr Colles shall not be neglected by myself; and should any results occur differing from the foregoing detail, they shall forthwith be communicated to the profession.

Lying-in-Hospital,
November 28, 1818.

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* Examining the umbilicus externally in several living and healthy infants, on the first and second days after the separation of the funis, I found great variety in the appearance of the papilla, fossa, &c. The papilla, varying however in size, was generally present, sometimes ulcerated, often like a healthy granulation. The surrounding fossa appeared ulcerated in some, in others red and healthy.

I have at present an infant under my care six weeks old, in whom the centre of the fossa is occupied by a large papilla, on which the extremities of the umbilical vessels open, and discharge a considerable quantity of a thick yellow fluid. The surrounding fossa is slightly ulcerated.

VIII.

Case of apparently Idiopathic Phrenitis. By ROBERT PALEY, M. D. Halifax. Communicated to Dr BATEMAN. *

MY DEAR SIR,

YOU may probably recollect, whilst I had the honour to be your colleague at the Public Dispensary, that I mentioned having had a case of apparently idiopathic phrenitis. Since I became a resident of this town, a case, in many respects similar, has occurred to me, of which I will give you the particulars; and should you deem it worthy of a place in the Edinburgh Medical and Surgical Journal, it is very much at your service.—I remain, my Dear Sir, very sincerely yours, ROBERT PALEY.

Halifax, April 18, 1807.

ON January 7th I was desired to visit a labouring man, æt. 40, who, about a month before, had been the subject of a sore throat, from which, however, he had been perfectly recovered upwards of three weeks. Three days ago, after having eaten a very hearty breakfast, he was suddenly seized with a most violent pain of the head. An apothecary was sent for in the evening, who declared it to be a case of putrid fever, and gave him some opening medicines, all of which were rejected. He then tried a mixture of kali and lemon juice, but with the same result. When I first saw him, the pain of his head seemed to be excruciating; and he described it as shooting through his brain from the right side to the left. His tongue was covered with a brown fur. Pulse 80, and feeble. There was not any excess of heat or thirst. Every thing which he took, whether food or medicine, was rejected almost immediately. The vessels on the tunica conjunctiva of the right eye were turgid; accompanied with intolerantia lucis, and an acute sense of hearing. He had been twice or three times delirious, and had not had a stool since the day before he was first attacked. I ordered his head to be shaved, washed with brandy, and then to be covered with a blister; his feet to be put in hot water; an enema purgans to be administered immediately; and to take pills consisting of calomel and jalap till the stomach was settled and the bowels opened, when the nurse was directed to give him a small quantity of gruel with wine frequently, also the following mixtures:

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* From the date annexed to this communication, it will be seen that it has been in our possession more than eleven years. It was laid aside because the Edinburgh Editor did not then sufficiently appreciate its value. The change which has taken place since we received it, in our opinions respecting the nature of fever, has now led us to attach so much importance to the observation, that we have great satisfaction in confessing our error, and at last laying it before our readers. It appears to us particularly interesting in several points of view. It proves that there is nothing peculiar in the nature of the present epidemic fever; that cases of cephalic fever occurred both in London and Halifax in 1807, or before; and that venesection was then, as now, of essential service in its treatment. It also furnishes a very instructive example of the manner in which the erroneous doctrines of the schools gradually yield to accurate observation and sound judgment. Dr Paley at first considered this case as typhus; but when he, with much discrimination, observed symptoms which indicated an inflammatory affection of the brain, he was led to look upon it rather as a case of idiopathic phrenitis; and, although he was taught to regard these states of disease as being inconsistent with each other, he saw the necessity of admitting a mode of their coexistence, and of combating both at the same time, or in immediate succession.

℞ Decoct. cort. Peruv. flav. ℥viiss.
Tinct. opii. camphorat. ʒss. M. Capt. coch. ij. tertia quâque horâ.

8th.—He complained this morning of the pain being very violent, and said that it was much increased by the application of cold or the slightest agitation. Had taken ℈i. of calomel, and ʒj. of jalap, which had procured him four stools; the blister had risen very well, and the mixture remained on his stomach. Pulse and tongue were nearly in the same state as yesterday; he had not been delirious. Ordered to continue the mixture and wine.

9th.—He thought himself something better, but had been delirious, and had not yet had any sleep. Said he felt refreshed by the mixture and the gruel in the wine, but the pain of the head remained equally violent.

Rep. pil. purg. enema et mistura.

About nine o'clock in the evening, he became very much worse. After having been two or three hours delirious, he had become perfectly comatose; his urine and stools came away involuntarily; his pulse was so feeble, or otherwise contracted, that I could with difficulty count it; it was about 70, and very irregular. Previously to his becoming insensible, he had roared out several times, the pain was so excruciating. His breathing was now laborious, his extremities were quite cold, with subsultus tendinum; in short, I felt no hesitation in declaring that he could not live many hours. I, however, with some difficulty, got half a tea-cupful of warm brandy into his stomach, which seemed to rouse his faculties for a short time. His pulse was rendered more perceptible; it was still irregular, but not frequent. In the course of an hour, the effects of the brandy had subsided. About the same quantity was repeated, but with less effect than before. I therefore now determined to try the opposite mode of treatment, and accordingly opened a branch of the right temporal artery. At first the blood came away very slowly, so that I doubted of the artery being opened; however, the drops became gradually more frequent, and in the course of eight or ten minutes, the blood came away in a small stream. When about two ounces had been taken, the pulse at the wrist became evidently stronger. After twice that quantity had come away, the stream was copious, and the artery was seen to beat forcibly. Altogether upwards of half a pound was taken. He became sensible during the bleeding, and said that he felt very little pain. He was ordered to take a small quantity of beef-tea frequently.

10th, 11 o'clock A. M.—His extremities had now become warm; pulse 75; the pain had returned, but was not so violent as before the bleeding; the vessels of the right eye remained turgid; he had not slept, but had been tolerably composed. The crassamentum of the blood was firm, but without any buffy appearance. Ordered to continue the beef-tea, and to take a solution of nitre every two hours. 8 o'clock P. M.—The pain had increased, and he had been more than once delirious; pulse 68. I ordered ℥iv. of blood to be taken from the temporal artery, which gave him immediate relief; he was also ordered to take a camphor draught with tinct. opii. gtt. xx. at bedtime.

11th.—Had slept in the night, and there was a gentle moisture on the skin; pulse 70; and the tongue was less furred. He had taken some coffee to breakfast. Contr mistura nitrosa. In the evening when I called he was quite delirious; pulse 64. The inflammation of the right eye had abated. I opened another branch of the right temporal artery, and took ℥v. of blood with the same happy effects as before. Rep. pil. purg et enema.

12th.—During the night, he seemed so languid, that the nurse gave him a little wine, soon after taking which he fell asleep, and awoke with a craving appetite, and felt much refreshed. A blister was ordered to be kept open on the back of his neck; he was also ordered to take ʒj. of pulv. cort. Peruv. flav. every four hours, and his bowels to be kept open by means of the pills. His strength now appeared to be so much exhausted, that I thought it necessary to allow him wine, but I more than once found it necessary to desist for a short time, and again have recourse to the antiphlogistic regimen, and I once had occasion to repeat arteriotomia. He was several times afterwards delirious for a short time. However, on the 25th, he awoke suddenly in the night, in consequence of feeling an unpleasant sensation in his right ear, which proved to arise from the bursting of an abscess. He supposed that about two table-spoonfuls of matter came away. From this time he recovered very rapidly, but he still feels some pain from any sudden agitation, whether of mind or body.

I am aware, that, in the treatment of this case, my practice must appear to have been inconsistent, and I confess, that, till the evening of the 9th, I agreed with the apothecary in thinking it to be a case of typhus. I however then saw that there was great local congestion, accompanied with considerable general debility; indeed, so far were those two seemingly opposite states of the body combined, that I have great reason to believe, if I had not taken away blood one hour, and almost the very next supported my patient's strength by wine, he would not at this time have been living.

Halifax, 18*th April* 1807.

IX.

Cases of Typhus Fever, with Observations. By A. Wood, Member of the Royal College of Surgeons, London.

Case I.—June 1st.—James Reid, a robust labouring man, after exposure to the infection of fever, began to complain, two days ago, of the usual symptoms of that disorder;—slight headach, pains in the limbs, indolence, transient flushings of heat, preceded by chilliness and rigors. This was the account I received of him; and, being unable to see him before the evening of the following day, I sent him some active cathartic medicine, with injunctions that he should abstain from all food and drink of a stimulating nature.

June 2d.—On visiting him, I found the medicine had operated briskly, producing numerous evacuations of a glarey reddish appearance. He had passed a restless night, talking to himself in an unconnected manner, and not answering questions until they had been repeatedly put to him. He had headach, thirst, a coated tongue, brown at its base, hot and parched skin, and a pulse beating 110, with a vibratory feel. I was informed, that, for a great part of the preceding night, he had complained heavily of pain, and a sense of fulness in his stomach and bowels; but this had left him after a severe fit of vomiting. Sixteen ounces of blood were taken from his arm; and a powder, containing a grain of calomel, three of ipecacuanha, and ten of jalap, ordered to be taken every four hours.

3d.—On this and the following day I had no opportunity of seeing him. There was said to be no material change in the symptoms, except that he felt considerable pain in the left side of the abdomen. The powders had answered the intentions for which they were given, operating copiously on the stomach and bowels;—they were directed to be continued.

4th.—The pain in the abdomen was described as being extremely severe, and a blister was prescribed for it. Constant nausea, vomiting on taking liquids, with frequent dejections from the bowels, were said to be the effects of the powders. In other respects, the attendants thought him better; the pain in his head being less violent, and his mind more composed.

5th.—I found him much improved. The pain in the abdomen, which, from his description, must have been very acute, had entirely left him in about two hours after the application of the blister, and had not returned. His tongue was becoming moist, and assuming a better colour, and his other febrile symptoms had much abated. A dose of sulphate of magnesia was directed to be taken every second morning, and the powders to be omitted. In a week he walked four miles to call upon me, and was then free from fever.

Case II.—June 2d.—Alice Eccles, a delicate young woman, living in a crowded and filthy court, from which fever had not been absent for nearly a year, had been confined a week when I first saw her. In addition to the usual routine of symptoms when fever has existed some time, she had intense thirst, and a constant burning pain at the pit of the stomach, aggravated by retchings to vomit, which were frequent. Her breathing was short and hurried, her tongue covered with a thick yellow fur, her skin hot;—she had confusion rather than pain of the head, and slight mental wanderings. Her bowels were said to be purged; but, on strict inquiry, I found the evacuations, though numerous, dark and scanty, of a slimy appearance, and fetid smell. I took ten ounces of blood from her arm, when she fainted; and directed two drachms of sulphate of magnesia every two hours, and five grains of calomel at bedtime.

3d.—The vomiting ceased, and with it the pain at the stomach, after taking two doses of the salts. She has had several motions of a similar character with those she had yesterday. Her head was hot and painful, and she had been slightly delirious during the night. Her tongue was coated with a thick covering of brown fur to its very tip; and the secretion from its surface hardened so speedily, that she was constantly employed in scraping it off with her teeth, and spitting it out in small round lumps. Her pulse was 120, and her skin excessively hot. The same powders were prescribed for her as in the former case.

4th.—The operation of the powders had been somewhat severe, though their effects on the complaint were such as to induce me to wish her to persevere in taking them. She had vomited repeatedly, and her stools were described as having been almost innumerable. At her urgent request, I consented to a dose being taken twice a-day only, with intermediate doses of sulphate of magnesia.

5th.—She was convalescent, all her symptoms being diminished in violence. She had passed a good night; sleeping soundly in the intervals between her calls to the night-chair. These calls had been frequent.

6th, 7th, 8th.—She continued to recover. The same plan was persisted in, gradually decreasing the quantity of medicine.

After this time I frequently saw her in the street, following her usual avocations.

September 20th.—The same woman came to me, wishing to be bled. On inquiring into her reasons for it, she complained of a heaviness and dull pain of the forehead, with want of sleep and appetite; and on making an attentive examination, it was evident she was again labouring under her former complaint. I bled her to the amount of 12 ounces, and desired her to take some opening medicine; but this she declined, having been, in her own opinion, "purged enough for her whole life." Since her last illness, she had been repeatedly exposed to contagion; or, rather, she had been living in an atmosphere thoroughly saturated with infectious effluvia, the house in which she resided, and generally the room in which she slept, having had one or more cases of fever in them, and ventilation entirely prevented by ingeniously contrived plans for the exclusion of "the cold."

24th.—I was desired to visit her. The disorder, since the 20th, has been gradually unmasking itself, and assuming a more decided character. She has now typhus of the simple kind completely developed;—headach, pains in the back and limbs, uneasiness at the stomach, the bowels confined, hot and dry skin, &c. The powders were directed every six hours.

25th.—The pain in the head is much increased, with frequent startings and moaning during sleep, and a disposition to ramble and complain while awake. She was bled to 10 ounces;—the powders continued, and three grains of calomel given every night.

26th.—The symptoms were much the same, and the same remedies were persevered in.

27th.—Though the violent excitement which was so prominent on the 25th is in some degree subdued, she still complains of pain in the head, and there is evidently great vascular irritation through the whole system. Far from being reduced by the plan which has been pursued, she speaks more boldly and distinctly, has greater power to raise herself in bed, and is, in every respect, stronger, more composed, and cheerful. Her tongue has a yellow streak of fur running along each edge to the tip, while the middle has the dry glazed appearance of a half-broiled beef steak; and the interstices between her teeth are filled with the same kind of matter which she was in the habit of scraping from her tongue in her former illness. She was again bled; and when 14 ounces of blood had flowed, she fainted. A solution of sulphate of magnesia, with antimonial wine, was ordered in place of the powders.

28th.—She complained of incessant but unavailing calls to the night-chair Half a grain of opium was added to the night dose of calomel, and the purging mixture omitted.

29th.—I was gratified exceedingly to observe the amendment which had taken place during the last two days: there was not an unpleasant symptom in the case which was not considerably relieved.

30th.—She continued convalescent. To omit the calomel and opium.

Oct. 1.—Since the 29th, she has had but trifling evacuations from her bowels; and this day she complained of headach. A dose of calomel and jalap was directed to be taken immediately, and repeated the following morning.

2d.—She described herself as quite well, but weak. In a few days she was out of doors.

In this case I have described a state of the tongue which I have very frequently observed in typhus fever. Sometimes, instead of the yellow streak on each side of the glazed surface, the edges of the tongue will have a florid red appearance, and be perfectly moist, giving an appearance as if they were painted with blood; at others, the whole surface of the tongue will present the same uniform, cracked, baked look, which I have compared to the lean part of beef half broiled. I am induced to notice this particularly, since it has become the surest guide I know of in forming an accurate prognosis; for, whenever I observed the gradual extension of a white fur on the tongue, I could with confidence assert, that an improvement would soon take place; though, on the most minute and attentive examination of every other symptom, not the least change or amendment could be detected. In the cases where recovery was rapid, the tongue would be loaded with a fur of this kind, of considerable thickness, in the space of a day or two, and, in many instances, it remained a week or ten days, after every other symptom, save debility, had disappeared. I have not been able to trace any regular connection between these varied conditions of the tongue and the nature of the affection, or the organ principally suffering, though I think it not improbable that such connection may really exist.

I am convinced, however, that the want of secretion from the surface of the tongue in these states of that organ, is no proof of a similar deficiency in the stomach and bowels; since I have repeatedly observed it to continue, and even to come on during a state of active purging. I think we are beginning to act more wisely in examining the tongue, less with reference to the digestive organs than to the vascular system.

Case III.—Sept. 23d.—Kean, a girl aged 13, I saw on the fifth day of fever. Her symptoms presented nothing of a remarkable character, except a severe pleuritic pain in the left side, near the region of the heart, most severe during inspiration, and attended with a dry, short, and frequent cough. I attempted to bleed her, but failed, from the unusual smallness of the vein, and the girl's timidity. I directed powders of a similar description with those in the two former cases, and recommended a blister; but its application was prevented by the officious interference of one of her mother's gossips.

24th.—The pain in the side was not felt after the operation of the second dose of medicine. She has been plentifully vomited and purged, and is full as well as yesterday. For the next three days my notes contain little information. She persevered in taking the powders, and was occasionally better and worse. The note for the 27th concludes with,—"Upon the whole, though she is not suffering severe pain, the febrile symptoms are not at all mitigated. She has a brown, coated tongue, hot skin, pulse 116, with constant moaning, and, when disturbed, delirious conversation." Sulphate of magnesia was prescribed in place of the powders.

28th.—She has had no sleep during the night, but has talked incessantly. Her pulse is 110, and her other symptoms the same as yesterday. Her bowels are perfectly open, large quantities of a stone-coloured fluid matter being frequently evacuated. Six ounces of blood were taken from her arm, and the salts continued.

29th.—The night has been passed quietly, though without much sound sleep. She seems recruited this morning, and answers questions more readily and rationally.

30th.—She continues better.

Oct. 1st.—Appearances are still favourable. There being considerable irritation of the stomach and bowels, from the long-continued exhibition of purgative medicines; two grains of calomel, and half a grain of opium, were ordered to be taken at bedtime, and the salts omitted.

5th.—Since the last report she has improved considerably. The opiate has always procured tranquil sleep, with a gentle moisture of the skin; and she wishes much to continue it. It was however omitted, and a dose of calomel and jalap prescribed to be taken in a few days. Soon after this time I saw her quite recovered.

Since the publication of Dr Clutterbuck's excellent speculative work on fever, the attention of pathologists has been forcibly directed to the examination of the brain, in their anatomical researches into its seat and nature; and it may be regarded as a circumstance strongly confirmative of the correctness of that gentleman's ideas, that morbid appearances of the brain are now described as existing in a far greater proportion of cases than formerly. Yet physicians are not unanimous on this head. In an useful essay by Dr Mills of Dublin, wherein the utility of blood-letting is placed in a striking and very advantageous point of view, an arrangement of the varieties of typhus fever is proposed from the organ principally affected, and which he denominates its seat. Agreeing with Dr Mills, "that in every fever there is an inflammation of some one or more of the viscera, or other parts," I cannot but think his conclusions somewhat premature, when he decides that its seat may be various, as evidenced by the superior urgency of the symptoms, and the more perceptible character of the consequent disorganization; for, since cases of dissection are still frequently related, in which there is said to have been no morbid appearance whatever, and as it is inconceivable that inflammation sufficient to cause death should take place in any organ, except perhaps the brain, without leaving cognizable signs of its pre-existence, is it not a rational deduction from Dr M.'s own premises, *that fever never occurs without* *local over-excitement*, that "cephalic fever" has in these instances destroyed the patient? If this be granted, it follows as an inevitable conclusion, that where inflammation of the thoracic or abdominal viscera has occurred, and proved fatal, an affection of the brain may have preceded and accompanied it, and contributed to its production, by increasing the general excitement of the system. The absence of pain in the head during the life of the patient cannot be estimated as bearing much on the question, when the low degree of sensibility of the brain is considered; and it is remembered that acute pain is not felt in inflammation of the substance of the lungs, or of the parenchyma of the liver, whilst inflammation of their investning membranes gives intense pain. Analogous to these is phrenitis, and also incipient hydrocephalus, in which there is inflammation of the process of pia mater lining the ventricles; and reflecting on the delicacy of the functions of the brain, and the facility with which they are abolished, I think that, until we possess a more intimate acquaintance with its healthy structure, it would be too confident a presumption in our own knowledge to say when it is sufficiently diseased to have been the cause of death.

Opinions approaching in some measure to those of Dr Mills have been promulgated by Dr Armstrong, in his judicious and discriminating "Illustrations." In one point, however, there is a difference of some importance in a theoretical light. While Dr Mills asserts that there is no fever without local inflammation, Dr Armstrong expresses his conviction that "inflammation is *not* its inseparable and essential constituent;" and afterwards concludes, that, fever being a disease of simple increased excitement, inflammation, if it come on, is to be regarded as a consequence, and not a cause. He attributes its accession, in "*almost every* case," to the application of cold, or "some predisposition in the part affected." A question, perhaps not an improper one, might here be proposed to Dr A. Will the state of general vascular excitement which you call fever, independent of its connection with local inflammation, or combined with it in any organ except the brain, account for the phenomena of typhus, particularly the striking and peculiar derangements of the nervous and intellectual systems? The latter part of the question is of course equally appropriate to Dr M.

It would be a work of supererogation to state the arguments that may be advanced in support of the notion, that, in all cases of fever, the brain is primarily inflamed, since it has been so elaborately, and, to me at least, satisfactorily done by Dr Clutterbuck. I will say nothing of the common or specific nature of the inflammation, for whichever way we move we are involved in equal perplexity;—if we call it common, we feel puzzled to account for its infectious properties, and its observance of peculiar and certain laws;—if specific, we admit our total ignorance of its real nature.

I come now to notice the cases which introduce this communication. I have selected them from a considerable number, partly because they exhibited those affections of the thoracic and abdominal viscera, which, in all probability, would have induced Dr M. to consider them as the seat of fever, and Dr A. the parts to which his remedies ought principally to have been directed; and partly because these affections were removed by means directed to the disordered state of the sensorium. In the first case, from the pain and tenderness to the touch of the abdomen, it might very naturally be concluded that inflammation existed in the peritoneum or intestines; yet this supposition is rendered dubious, from the bowels being all the time amenable to the operation of purgatives, and the sudden removal of the pain on the application of a blister. In case the second, the stomach showed symptoms of active inflammation; but they were removed by one bleeding, *ad deliquium*, and opening medicines. And in the third case, where the symptoms of inflammation were most decided, the patient recovered from them, though positively nothing was done that could affect them in a direct manner. Reasoning from these facts, whose number I could readily increase, it seems a necessary conclusion, either that the pain and other symptoms were mere depraved sensations, arising from sympathy with the morbid condition of the brain, or that inflammation of these parts, occurring with typhus fever, is capable of being removed by means which would be totally inadequate in its uncombined form. I incline to the former of these opinions, from the frequency with which symptoms indicative of a pleuritic or hepatic affection attack patients a few days before death, and continue unsubdued till that event takes place, when, on examination, no traces, or, at most, very trivial ones, of previous disease are discovered. Whether or not we admit, with Dr Darwin, that sensation is the result of actions in the extremities of nerves, common experience will bear us out in asserting, that a continuance of inordinate or depraved sensation will occasion an increased determination of the blood to the part, and in the end lead to the establishment of indisputable inflammation. These local and accidental symptoms *seem* in many instances to demand a mode of treatment little suited to the other circumstances of the case, and are apt to withdraw our attention from the less obtrusive, but in general far more dangerous brainular affection.

However much we may feel disposed to dispute the correctness of Dr Hamilton's explanation of the *modus operandi* of purgative medicines in fever, we cannot but admire the candid and unassuming manner in which it was introduced into public notice. It is now, I presume, an almost universal conviction, that their beneficial effects do not arise so much from their merely removing irritating matter, as from the powerful qualities they possess of correcting disordered, and increasing diminished secretion of the organs concerned in digestion. If the influence of the brain over the stomach and bowels appear striking and remarkable, it is not the less wonderful how speedily a change in the secreting surfaces of those parts operates back again upon the brain; and nothing but a belief in the general acknowledgment of the fact prevents me relating some interesting cases described in Mr Abernethy's admirable Lectures. Next to blood-letting, I hold the exhibition of purgatives to be the most efficient remedy in fever. Besides the immediate counter irritation on the brain, produced by them, they are serviceable in withdrawing no inconsiderable quantity of fluid entirely out of the circulation; and more than this, like every other means which produces a powerful impression on the system, they appear occasionally to arrest the progress of fever, by breaking into and destroying the association of morbid actions which constitutes its formation. By joining emetic to purgative ingredients, a more *extensive* operation is insured; and I have usually added calomel, in the hope that its peculiar effects on the liver, and on secretion at large, would co-operate in the general intention. It is not the giving an emetic in the commencement of the disease, and a purge for a few mornings after, that I intend to advocate, but the keeping up for two or three days a never-ceasing action, by regularly repeated doses.

In mentioning the opinions of individuals, I trust that I have never intruded on the respectful distance which ought always to be observed in canvassing the doctrines of eminent men. The intelligent author of the Notes to the fourth canto of Childe Harold has an extract, concluding with sentiments as applicable to philosophy in medicine as in religion or politics. "Philosophy, wisdom and liberty support each other; he who will not reason, is a bigot; he who cannot, is a fool; and, he who dares not, is a slave." However damning may be some of the sins of the present race of medical men, they will at least escape the imputations of profound and slavish submission to names and nonsense, with which their predecessors have been not unjustly taxed.

Rochdale, Oct. 18, 1818.

X.

History of Continued Fever in Leeds, preceded by Observations on the Medical Police of the City. By ADAM HUNTER, M. D. Physician to the House of Recovery.

SOON after being appointed Physician to the House of Recovery at this place, which happened about six months ago, I resolved to transmit you an annual report of the cases which came under my care. Having lately, however, had the honour of seeing a letter from a gentleman in Glasgow to one of our most intelligent and public spirited inhabitants, respecting the measures adopted here for the prevention and cure of typhus fever; and learning from the same respectable source, that you had wrote for an abstract of the cases for the last two or three years, I have been induced, by your own very appropriate quotation, "better I had ane imperfect account transmitted, than none at all," to draw up the following statement without farther delay. I trust the earnest wish I have to afford any little information in my power, for the public good, will sufficiently excuse, with yourself and your readers, the desultory nature, and other imperfections of this communication.

As I am not aware that any account of the medical police of Leeds has yet found a place in your pages, and as it has frequently occurred to me that medical statics are too much neglected in the history and treatment of disease, the following short description of the situation, population, and employments, with the manners and customs of the inhabitants, may not prove uninteresting. For it is only by a diligent and attentive investigation of such particulars, with the publishing and comparing the general results of our public hospitals, that we can attain to any accurate conclusions in this department of medicine.

The town of Leeds, in lat. 53° 47′ 33″, long. 1° 38′ 30", is situated in the West Riding of Yorkshire. Extending along the north bank of the Aire, it covers an eminence gently rising from that river to the upper end of the town, and declining with an easy slope to the east and west. On the east the town falls into and again rises from the other side of a low valley, through which passes a small brook, having on its banks a great number of dyeing-houses and other manufactories. The town is in general well built, almost entirely of brick; but its different quarters form in many places a striking contrast. In the eastern part, on the banks of the above mentioned rivulet, the houses are mean, the streets and lanes dirty, crooked and irregular; and, in the words of a work now before me, (Bigland's Beauties of England,) "emitting disagreeable smells from the dyeing-houses and different manufactories, while the discoloured and dirty torrent reminds us of the waters of Styx and Cocytus."

> "Cocyti alta vides Stygiumque paludem."

Contiguous to the town, on the south side of the Aire, and joined to it by a bridge, lie the populous and extensive villages of Holbeck and Hunslet, which, with the exception of being built upon a plain, approximate in every respect to the appearance of the eastern part of the town.

The parish of Leeds forms a square of fully seven miles in diameter, and thirty miles in circumference; is divided into ten townships, exclusive of the township of Leeds; and by the census of 1811, contains 62,534 inhabitants; of which belong to the town of Leeds 35,951. The population of the whole parish has of late, however, been rapidly on the increase, and may now perhaps be estimated at nearly 70,000.

Westerly winds are most prevalent; and from the low situation of the town, compared with the adjacent country, it is frequently covered with a dense thick fog. This, joined with the volumes of smoke vomited from the chimneys of the numerous manufactories, forms a dark black cloud, which envelopes the town; and which, when viewed from the surrounding heights, the eye in vain attempts to penetrate.

The river Aire supplies the town with water, which, from the contamination it receives in its passage through the factories, is brought by means of a tunnel from a short distance above, and conducted into reservoirs, whence, after remaining some time to depurate, it is distributed by leaden pipes into every part of the town, and almost into every house. The spring water, though rather hard, and from a calcareous soil, is pure and wholesome.

The soil is a strong, loamy clay, covering beds of coal, of which there are immense quantities taken up in the immediate vicinity. The rising grounds furnish strata of sandstone and granite. There is no wood of any consequence within some miles.

Leeds is entirely a manufacturing town. In 1800, the population of the parish was given at 53,162. There were 20,367 stated to be employed in the trade and manufactories, particularly of woollen cloth. Lately, however, the spinning of flax has gained ground with astonishing rapidity. This branch employs a greater number of women and young people than the woollen manufacture; consequently, the number now engaged in these occupations must bear more than an equal proportion with our increased population. The comparative healthiness of the two I have not yet been able to ascertain to my own satisfaction; though I believe the preference is generally allowed to the woollen manufacture. As the recent and still pending discussions in our national council excited general curiosity, I have been led to pay some attention to this important subject; and the result of my personal observation and inquiries is, that little more can be done than what is already accomplished, to ameliorate the condition of this portion of our population. In the large factories the principles of ventilation and cleanliness seem perfectly understood and acted upon. I have repeatedly stationed myself at the door of these immense depots of our national wealth, and witnessed the egress of their numerous inmates; and so far from appearing to pine, or be in misery, they uniformly seem to enjoy the utmost hilarity. Neither do their faces or general appearance indicate any very injurious consequences from their confinement, much less, indeed, than I had previously supposed; and I shall venture to affirm, that, on taking any number of children from a factory, and comparing them with an equal number of children, taken indiscriminately from the families of our grocers, tailors, and other handicraft and individual trades, the ratio of health and vigour, as well in reality as in appearance, will be found decidedly in favour of the former. Both, indeed, partake of a grosser diet than what I consider conducive to the health of children; for, by how much the parents of the one class are richer than the parents of the other, by so much are their children crammed with rich and indigestible food. The want of exercise, I would also say, is on the side of the children of mechanics. The number of pale, sallow, scrofulous children, I find generally on entering the houses of the latter, proves this almost to a demonstration; and although the etiolated appearance of neither can compare with the florid and robust child of the weather-beaten mountaineer, still the factory child approaches the nearest, and not unfrequently enjoys better health and complexion than the children of his master. To conclude this part of my subject in reference to fever, out of 135 cases admitted in the House of Recovery during the last six months, I do not find above seven from the factories, two of which were Irish girls, lately arrived in Leeds.

Most of the townships in the country have what is termed a "Town's Doctor," generally the junior surgeon of the place, who, for a small salary, visits the poor receiving parish relief; this is a branch, and not the least useful, of that enormous tree of poor rates which overshadows all England. Each township has annually a scene of festivity termed a wake or feast, which bears a near resemblance to the fairs held in the villages in Scotland, with this exception, that pleasure, not merchandise, is the avowed intention of the former. I have not hitherto been able to procure any satisfactory information of their origin, though undoubtedly of very ancient date; but their importance in a medical point of view is much more interesting; for, though these meetings are sufficiently licentious and dissipated, still there is thorough "scouring out," and general whitewashing previous to the important day: and as the solemn sacramental occasion of the north sees the peasant and his family annually arrayed in a new suit, so the Yorkshire feast is the signal for the new-rigging of many a tatterdemallion, whose fringed drapery might otherwise wave in the wind for some months longer.

Leeds partakes of most of the virtues, and some of the vices incident to large manufacturing towns. The morals of the inhabitants in every sphere of life are generally very imposing. An unremitting attention to business, to the almost entire exclusion of every other object, and unbounded and almost enthusiastic exercise of that branch of charity which may be ranged under the head of public contribution, are two no less prominent than praiseworthy features in our inhabitants. Public amusements of every description, which have frequently no inconsiderable effect over disease, are viewed with a jealous eye, and strenuously condemned by some, more especially those which require any exertion in the muscles of voluntary motion, as tending perhaps to derange the centre of gravity so necessary for commercial calculation. Be this as it may, the honest physician and real philanthropist will rejoice to see the dejected mind roused into cheerfulness, and the sallow and care-worn countenance tinged with the heightened glow of health, by those simple and innocent measures, which the Author of Nature has put in the power of the humblest, as well as the most wealthy of our species.

Perhaps in no town in England are charitable institutions more liberally and universally supported, of which we can boast a number equal, if not superior, to most places of the same size. Besides the House of Recovery, the more immediate object of this paper, our General Infirmary is worthy of particular notice, as inferior to few in extent and usefulness, as well as in the able manner in which I believe it has always been conducted. This handsome edifice is 150 feet in length, by 38 in breadth, with two large wings. The wards are 15 feet 8 inches high, and divided in the most judicious manner. Circular apertures, for the purpose of ventilation, open from a passage 5½ feet wide into each ward. It was opened for the reception of patients in March 1767, since which period, to the 29th of September 1817, when the last report, now before me, was published, 76,386 patients have been admitted, of which 44,495 have been cured; 3,141 have died; and 6,372 have been relieved; besides, it is well known, says the report, (alluding to the 22,478 not accounted for,) "that the greatest part of those discharged for non-attendance have been either cured or greatly relieved." Of the whole, perhaps a majority were out-patients, to whom advice and medicine were given at a certain hour in two days of the week; in this way answering the purposes of a dispensary. It contains upwards of 100 beds. Was built, it is supposed, by subscriptions, legacies, &c. The annual expence may amount to about L.3,000. Infectious fevers and the venereal disease are the only cases not strictly admissible; to the last of which private feeling, rather, I conceive, than public propriety, has denied this place of cure. The following report for one year may not be unacceptable to many of your readers.

GENERAL INFIRMARY AT LEEDS,

Report of Patients Admitted and Discharged from Michaelmas 1816, to Michaelmas 1817.

In-Patients.		
Remained on the Books,	554	
Admitted during the year,	899	
		1253
Discharged Cured,	424	
Relieved,	69	
At their own Request,	27	
Incurable,	6	
Irregularity,	18	
Dead,	46	
Non-attendance,	250	
Refused Operations or Medicines,	5	
Improper Objects,	2	
Remain under Cure,	406	
		1253

Out-Patients.		
Remained on the Books,	867	
Admitted during the year,	1872	
		2739
Discharged Cured,	1181	
Relieved,	150	
At their own Request,	24	
Dead,	44	
Incurable,	2	
Non-attendance,	456	
Improper Objects,	1	
Refused Operations or Medicines,	1	
Remain under Cure,	900	
		2739

A detail of the various items of expenditure would be superfluous; the general amount for the above year was L.4101, 10s. 9d. There is no statement published of the various diseases; which, with yourself, I consider a desideratum in the reports of this, and all similar institutions. From the want of this guide, it is less easy to say what diseases are most prevalent, or peculiar to Leeds; but of those which assume the chronic form, the different varieties of struma have hitherto most commonly presented themselves to my notice.

Having so much before me, it would be unpardonable to insist further on our general policy; but I may mention, *en passant*, that public baths are about to be constructed, on a scale suitable to their importance in a town similarly situated as Leeds; and were our very intelligent magistrates and citizens, who are so feelingly alive to every useful improvement, to conjoin with the baths a piece of ground laid out into walks for the general use, it would contribute no little to the health, comfort, and amusement of a sedentary people, in their hours of relaxation.

From the foregoing details it must be apparent, that when contagious fevers arise, or are otherwise introduced, they must necessarily spread with rapidity. Accordingly, I find the town of Leeds has in former times suffered severely from such diseases. It must be in the recollection of many, and I presume, in the knowledge of all your readers, that the harvest of 1799 was uncommonly bad and defective. Typhus fever, the never-failing follower of famine, made its appearance in Leeds the ensuing season with more than wonted severity; and though every exertion was made, as well by the faculty as the inhabitants, nothing could resist its progress. The then recent examples of Chester and Manchester at length directed the public attention to the erecting a House of Recovery; and although some unaccountable bias existed in the minds of many otherwise intelligent individuals against such institutions, though in a lesser degree, yet, by the energy and perseverance of my colleague Dr Thorp, seconded by my friend Dr Hird, and by the good sense of a majority of the inhabitants, a House of Recovery was erected, than which nothing has ever proved, or is likely to prove, more beneficial to the town of Leeds. I have before me a short tract published by Dr Thorp in 1802, on the fever at that period, containing, besides, some judicious reflections on the means to be adopted for guarding against and checking the progress of infection. "In a visit," says the Doctor, "made a few days ago to these abodes of misery, I saw, in one particular district, upwards of twenty-five families ill in contagious fever. In some houses two, in others six or seven were confined, many of whom appeared to be in extreme danger."

"From a minute investigation into the nature of their complaints, I may safely assert, that a great majority of these might have been prevented by a timely removal of the first infected into a fever ward."

And Mr Moxon, at that time superintendent of the sick poor, states to the Doctor, " That sixty families in epidemic fever are under his care at this time. New applications are daily making. In some families three, in others six or seven, are in disease. Forty persons in fever have applied to him for medical aid within the present week." Such facts were not to be resisted. The cause of humanity prevailed. And the House of Recovery was open for the reception of patients in the month of October 1804, since which period, though, as may be expected, continued fever has never entirely left us, it has, notwithstanding, been kept decidedly at bay.

The House of Recovery is a lofty building, about 80 feet in length, by 45 in breadth, consisting of three floors, besides the cellars and attics. The ground floor is occupied for domestic purposes, and contains the kitchens, board-room, laboratory, bath-room, &c. The second and third floors are solely appropriated for the reception of patients, the number of which has never exceeded 25 at one time, though the house is capable of containing double that number. The ceilings are 16 feet high, with two suites of rooms on each floor, separated by a spacious gallery, from which apertures open into the rooms, similar to those of the Infirmary. The whole admits of the most perfect ventilation, which is daily enforced in the strictest manner.

The absence of our worthy treasurer Charles Brown, Esq. at Bath, who, with his friends, was one of the most active promoters, and hath ever since continued a liberal supporter of the institution, renders it impossible for me to send you any correct statement of the funds, but the general amount, including salaries, is about L. 2 for each patient.

The following tables, taken from the reports published annually, and carefully collated with the secretary's book, will show the progress of fever in the House of Recovery at Leeds, from its commencement on the 1st of November 1804, to the 1st of November 1818, 14 years. The first column (A) marks the number of admissions, the second (C) the number cured, the third (D) the number dead, and the fourth (R) the number sent out at their own request, or who were improper cases for the house.

		A.	C.	D.	R.
1804	Nov.	5	5	0	0
	Dec.	5	5	0	0
1805	Jan.	1	1	0	0
	Feb.	5	4	1	0
	Mar.	2	1	0	1
	April	4	4	0	0
	May	2	1	0	1
	June	1	1	0	0
	July	7	6	1	0
	Aug.	6	5	1	0
	Sept.	8	8	0	0
	Oct.	10	9	1	0
	Nov.	9	8	1	0
	Dec.	11	10	1	0
1806	Jan.	6	6	0	0
	Feb.	14	14	0	0
	Mar.	10	10	0	0
	April	3	3	0	0
	May	2	2	0	0
	June	5	3	2	0
	July	3	3	0	0
	Aug.	2	2	0	0
	Sept.	4	4	0	0
	Oct.	8	8	0	0
	Nov.	9	9	0	0
	Dec.	9	9	0	0
1807	Jan.	9	7	0	2
	Feb.	4	3	1	0
	Mar.	2	2	0	0
	April	5	5	0	0
	May	3	3	0	0
	June	2	2	0	0
	July	4	4	0	0
	Aug.	0	0	0	0
	Sept.	3	3	0	0
	Oct.	0	0	0	0
	Nov.	0	2	0	0
	Dec.	0	0	0	0
1808	Jan.	0	0	0	0
	Feb.	3	3	0	0
	Mar.	2	2	0	0
	April	3	3	0	0
	May	11	11	0	0
	June	6	6	0	0
	July	5	5	0	0
	Aug.	5	5	1	0
	Sept.	11	11	0	0
	Oct.	11	10	1	0
	Nov.	11	11	0	0
	Dec.	12	10	1	1
1809	Jan.	8	8	0	0
	Feb.	12	11	1	0
	Mar.	7	3	4	0
	April	5	3	1	1
	May	15	10	0	1
	June	1	8	0	0
	July	8	1	0	0
	Aug.	1	4	1	0
	Sept.	4	4	0	0
	Oct.	7	7	0	0
	Nov.	10	9	1	0
	Dec.	15	15	0	0
1810	Jan.	7	6	0	1
	Feb.	5	5	0	0
	Mar.	3	3	0	0
	April	8	5	3	0
	May	11	9	2	0
	June	8	6	2	0
	July	5	4	1	0
	Aug	3	3	0	0
	Sept.	6	4	2	0
	Oct.	5	5	0	0
	Nov.	7	5	2	0
	Dec.	7	5	2	0
1811	Jan.	9	9	0	0
	Feb.	11	10	1	0
	Mar.	8	8	0	0
	April	12	12	0	0
	May	10	10	0	0
	June	6	5	1	0
	July	6	6	0	0
	Aug.	4	2	2	0
	Sept.	7	7	0	0
	Oct.	3	3	0	0
	Nov.	5	5	0	0
	Dec.	11	11	0	0
1812	Jan.	6	4	2	0
	Feb.	10	8	2	0
	Mar.	6	4	2	0
	April	1	1	0	0
	May	0	0	0	0
	June	6	5	1	0
	July	6	6	0	0
	Aug.	6	5	1	0
	Sept.	5	5	2	0
	Oct.	5	5	0	0
	Nov.	9	8	1	0
	Dec.	22	21	1	0
1813	Jan.	12	10	2	0
	Feb.	11	10	1	0
	Mar.	5	4	1	0
	April	5	8	1	0
	May	9	3	0	0
	June	15	15	0	0
	July	11	9	2	0
	Aug.	19	17	2	0
	Sept.	11	11	0	0
	Oct.	10	9	1	0
	Nov.	12	11	0	1
	Dec.	19	18	1	0
1814	Jan.	6	6	0	0
	Feb.	7	7	0	0
	Mar.	6	5	1	0
	April	2	2	0	0
	May	8	8	0	0
	June	5	5	0	0
	July	6	6	0	0
	Aug.	5	5	0	0
	Sept.	6	5	1	0
	Oct.	13	12	1	0
	Nov.	9	8	1	0
	Dec.	6	6	0	0
1815	Jan	5	5	0	0
	Feb.	7	6	1	0
	Mar.	9	8	1	0
	April	19	18	1	0
	May	12	10	2	0
	June	10	7	3	0
	July	8	8	0	0
	Aug.	24	21	3	0
	Sept.	22	20	1	0
	Oct.	14	12	2	0
	Nov.	9	9	0	0
	Dec.	7	6	1	0
1816	Jan.	7	6	1	0
	Feb.	9	8	1	0
	Mar.	6	6	0	8
	April	12	9	3	0
	May	11	10	1	0
	June	7	6	1	0
	July	10	14	2	0
	Aug.	14	14	0	0
	Sept.	8	7	1	0
	Oct.	12	11	1	0
	Nov.	8	7	1	0
	Dec.	11	10	1	0
1817	Jan.	17	15	2	0
	Feb.	8	8	0	0
	Mar.	11	11	0	0
	April	15	15	0	0
	May	7	7	0	0
	June	7	7	0	0
	July	6	6	0	0
	Aug.	10	9	1	0
	Sept.	24	24	0	0
	Oct.	28	27	1	0
	Nov.	18	17	1	0
	Dec.	27	24	3	0
1818	Jan.	50	49	1	0
	Feb.	18	17	1	0
	Mar.	23	21	2	0
	April	28	26	2	0
	May	17	13	4	0
	June	10	7	3	0
	July	16	13	3	0
	Aug.	21	19	2	0
	Sept.	31	30	1	0
	Oct.	40	39	1	0

	A.	C.	D	R.
1804 Nov. & Dec.	10	10	0	0
1805 whole year.	66	58	6	2
1806	75	73	2	0
1807	35	32	1	2
1808	80	76	3	1
1809	93	83	8	2
1810	75	60	14	1
1811	92	88	4	0
1812	80	68	12	0
1813	137	125	11	0
1814	79	75	4	0
1815	146	131	15	0
1816	121	108	13	1
1817	178	170	8	0
1818 first 10 months,	254	234	20	0
Grand Total,	1521	1391	120	9

Cured in the proportion of 12½ to 1

In surveying this table, it may be thought that the number shown to be admitted at the beginning is little commensurate with the previous outline of the epidemic. But, as too often happens in particular as well as in general instances, the fever had spent its force ere the remedy could be applied. And it may also be well to bear in mind, that, from the hostility manifested at its establishment, the populace imbibed the most ridiculous prejudices against entering within its walls, or even approaching within a certain distance. In this way, too, we may account for a gradual increase of patients without a similar increase in the fever.

TABLE II.—This anagram of Table I. may perhaps be satisfactory. It contains the admissions, cures, deaths, and irregular patients in the respective months, for 13 years, viz. from January 1st 1805, to January 1st 1818, during all which period the fever could not be called epidemic.

	A.	C.	D.	R.
January	93	83	7	3
February	106	97	9	
March	77	67	9	1
April	98	88	9	1
May	91	84	5	2
June	86	76	10	
July	84	78	6	
August	103	91	12	
September	119	112	7	
October	124	116	8	
November	118	110	8	
December	157	145	11	1

From this table, December seems to have most fever, and, with the exception of May and September, most cures in proportion. The summer months fewest cases, but most deaths; June and August most fatal. This in some measure verifies Dr Bateman's calculations as to the fatality of fever when the cases are fewest. But I conceive more depends upon the season than the number.

TABLE III.—To the proportions sent by the different Townships in the first 1216 cases, with their respective population.

	No.	Population.
Leeds,	726	
Soldiers,	11	
Workhouse,	7	
Infirmary,	6	
House of Recovery,	4	
Total,	754	36,105
Hunslet,	139	6,393
Holbeck,	93	5,124
Beeston,	31	1,026
Headingley,	5	1,670
Potter-Newton,	4	854

None of these townships are above three miles from Leeds; Holbeck and Hunslet are the nearest. Those from the infirmary were either patients or servants of the institution. Four, marked House of Recovery, were servants; these, out of four servants regularly retained, are all that have caught the fever in the house. None of the medical officers have ever been infected, if we except, perhaps, the late Dr John Thomson, who fell a victim to this fever early in the spring. In the ardour and knowledge of his profession he has perhaps left few equals.

TABLE IV.—Contains the Age and Number affected in the first 1000 Cases.

Age.	No.	Age.	No.	Age.	No.	Age.	No.	Age.	No.	Age.	No.
1	2	16	24	31	9	46		61	1	76	
2	2	17	34	32	6	47	5	62		77	
3	3	18	38	33	7	48	3	63	1	78	1
4	10	19	39	34	9	49	1	64	2	79	
5	3	20	26	35	16	50	11	65	1	80	
6	20	21	28	36	8	51		66		81	
7	14	22	26	37	3	52	4	67		82	
8	20	23	28	38	7	53	6	68	1	83	
9	27	24	15	39	4	54	1	69		84	
10	28	25	16	40	12	55	1	70	2	85	
11	28	26	13	41	2	56	1	71		86	
12	37	27	11	42	3	57		72		87	
13	36	28	7	43		58		73		88	
14	37	29	9	44	2	59		74		89	
15	35	30	20	45	6	60	4	75		90	

This table was presented to me by our worthy Secretary, Mr Matthew Talbot, author of an useful and laborious work, the Analysis of the Bible, who, if Plutus was always allied to Minerva, ought to hold a much more lucrative situation.

There is one imperfection in this table, and indeed in all the others; the cases of scarlatina, not being distinctly marked, are not separated from those of typhus. But out of 299 patients admitted during last twelve months, five only were cases of scarlet fever.

Proportion of Males and Females admitted in the first 1000 cases.

Males, 461	Females, 539

On these tables, it is not my intention to offer any remarks, as none of the cases were under my care till the last six months. Several useful inferences, however, may be drawn from them; and when we consider that none of the cases are subtracted which proved fatal, and which, though admitted, were actually not cases of fever, * I believe they will bear a comparison with most reports of the kind with which I am acquainted.

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* From the number of medical gentlemen who may sign the admissions, and the incipient symptoms of typhus being sometimes slightly marked; it occasionally happens, that cases not purely febrile are admitted, more especially those labouring under low hectic fever, from chronic derangement of the

It will be seen, by referring to Table I. that an evident, though not excessive increase of fever has taken place in Leeds, as well as most other towns, within the last 14 months. This I shall briefly endeavour to account for, and conclude with a very few observations on the mode of treatment. It appears to me indisputable, that the present epidemic may be traced to the distress suffered by the lower Irish in the winter of 1815, and the same cause may perhaps be sufficient to account for its almost simultaneous origin and progress in most of the large towns in Scotland, and in some districts in England; but, without carrying our views directly in opposition to facts, we cannot, in this way, account for its increase in Leeds, as it did not begin for nearly two years after the greatest distress was over. Besides, the lower orders in Leeds, and in most other places in Yorkshire, where woollen manufactures prevail, did not suffer in nearly the same ratio as in most other parts of the kingdom. The woollen trade is less liable to fluctuation than almost any other branch, consequently the wages, naturally high, remain more at par; and when a depression does take place, the enlightened policy of our manufacturers induces them to keep their people at work, without attempting to annihilate the wages; conscious that, though their profits are less, they would otherwise have to support the same men in idleness. Were this plan more universally acted upon, it might be better for all parties. From these and the following considerations, I am inclined to refer the present fever more directly to infection, to the most fertile and hitherto permanent source of which I have this day had the satisfaction of witnessing the axe laid to the root;—I allude to the low miserable lodging-houses which exist in all large towns, and which will always be found the never failing reservoirs of vice, contagion, and disease.* Of the 254 patients admitted into the House of Recovery during the last ten months, 66 came from those lodging-houses, and the same

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thoracic or abdominal viscera, and being often too ill to be removed, they generally prove fatal.

* For a more detailed account of the measures adopting, I can at present refer your readers to a similar plan now in force at Kendal. It consists in having a vagrant office, where mendicants from other quarters, applying for relief, or found begging in the streets, are examined; if proper objects, relieved, lodged one night, and then conducted beyond the precincts of the town. In this way they are prevented from coming into contact with the mass of the population. The greater freedom enjoyed by mendicants in Scotland, in consequence of the non-existence of poor-rates, may perhaps render similar measures more difficult to be applied with the same powerful effect; but it is worthy of serious consideration, as, from my own personal knowledge of Edinburgh and Glasgow, this evil is not less prevalent in these towns than in Leeds.

proportion may be observed in the preceding four months of 1817; of that number, upwards of 50 were strangers. Where such a focus of infection is permitted to exist, in vain shall we labour to stem the ravages which typhus fever is committing amongst us. To prove this still further, I may observe, that of the 50 cases brought to the House in January last, 20 came from four or five lodging-houses in a low and dirty street, (as its names implies,) Marsh Lane; and from another place equally bad, Boot and Shoe Yard; the rest of the cases in that month were principally from the houses and streets adjoining, or from similar streets, the inhabitants of which may naturally be supposed to have had communication with the former. The remainder of the 66 cases continued chiefly to pour in upon us during the months of February, March, and April, about which time it got a footing in Hunslet, Holbeck, and the adjoining villages, and it was not before the month of June, that we received any number of cases from the west end, or any other airy part of the town. About this time, too, it became more frequent in private practice.

I am not yet sufficiently acquainted with the atmospherical changes in Leeds, to be able to adduce any very important inferences from this source. The month of April was moist, raw, and cold, yet the deaths were only 2 in 28. In May, June, and July the weather extremely hot and sultry; we had no rain, and the air (by those who have felt both) was said to resemble that of an African climate. In those months the fever assumed decidedly a more fatal type, most of the cases coming under the denomination of typhus gravior. By referring to Table I. you will perceive, that out of 43 cases admitted 10 died; two of these, indeed, were not cases of fever, but this does not affect the general statement. The month of August and early part of September were equally arid and sultry; and were also distinguished by a more universal attack of cholera morbus than has almost ever been remembered in Leeds. Though severe in many, and in several cases attended with such sudden sinking of the vis vitæ, that raw brandy was had recourse to by some practitioners, in only a very few instances did it prove fatal. Cholera is at all times the disease of those months; the excessive hot weather, assisted perhaps by the immense quantities of fruit consumed during the summer, may enable us to account for its universality. Most of the fever cases which at this time came to the House, were persons previously weakened by the cholera, in two of which, both girls under my own immediate care, did it prove fatal. The latter part of September and the whole month of October was uncommonly mild. Gentle equinoctial showers now revived the parched fields; all nature seemed to rejoice; and the general temperature in every respect resembled what we usually enjoy in May or June. The fever too assumed the same mild form; the cholera cases were exhausted; and out of 71 patients in those two months, we did not lose one in pure fever; for of the two marked dead, one, an old man of sixty six, was brought in labouring under and died of peripneumonia notha; the other, under Dr Thorp, sunk under some other chronic disease.

In no one prominent symptom has the fever in Leeds differed from the many accurate histories detailed in the late numerous publications on this subject. Pain in the head and back, extending to the limbs, suffusion of the eyes, general oppression, nausea, and occasional fixed, and if I dare use the term, spasmodic pain in the bowels, are the leading features. The pulmonic affection has been very prevalent, especially after the violent symptoms have been subdued. Deafness has also supervened more frequently than I ever remember to have seen in the same kind of fever. In a few instances, discharges of black grumous blood from the bowels have occurred, though without any fatal result. In many of the cases, the stages so distinctly pourtrayed by Drs Armstrong and Bateman, have been sufficiently evident. In only one case, to my knowledge, has sloughing taken place. Petechiæ have appeared in none, though I have heard of a few instances of their appearance in private practice. A further enumeration of symptoms would be superfluous. And, for the same reason, a very few observations on the method of cure will suffice, as it coincides in almost every respect with that delivered in the late valuable treatise by Dr Bateman.

On the appearances from dissection I can offer few remarks, as, unfortunately, the procuring this most valuable part of knowledge is attended with the utmost difficulty. In only two instances have I seen it accomplished here. One, a robust plethoric man, aged 40, died about the 14th day of the fever, quite delirious. He had, I believe, been once bled during the attack. The brain appeared actually loaded with blood; the vessels of the membranes were distended as if fully injected. No other particular derangement was evident. The other, a woman about 30, had been conveyed, very ill of the fever, in an open cart from a distance of 24 miles, in one of the hottest days I remember last summer. When I first saw her she appeared literally scorched. She lingered two days and died. The brain did not altogether exhibit such inflammatory appearances as I had expected, but there was sufficient distention of the vessels to shew that increased action had occurred. The serum was in greater apparent quantity than in the former case. In neither could an examination of the other viscera be obtained. Had I before entertained any doubts of the absolute necessity of venesection, even in the second stage, where oppression is evident, these would materially have tended to remove them.

When a patient is brought to the House of Recovery, he is immediately taken to the bath-room, stript, washed, and conveyed to bed. The apothecary generally sees him first, and except his bowels have been previously freely opened, immediately exhibits a calomel bolus of five grains, followed up by a solution of sulphas magnesiæ; and, in the language of Dr Bateman, it is frequently astonishing, on visiting him the next morning, to witness the change produced by a good and airy bed and clean linen, in conjunction with these simple measures. On inquiring how he is, the answer almost universally is, he feels better than when he came. If the headach is severe, he is bled from the arm or temples, with the lancet, cupping-glasses, or leeches, as the violence of the symptoms may indicate. If nausea is present, or where the headach seems to depend upon derangement of the stomach, an emetic is exhibited, followed up by a regular and pretty free action upon the bowels; and where the head is not relieved to the full extent, or becomes secondarily affected, or where local congestion is present in any other part, a blister is applied behind the neck, or upon that part where the inflammation exists, which, in almost every instance, is attended with the happiest effect. The drink is toast and water, barley-water, or plain water, which is frequently preferred, and given *ad libitum.* Frequent sponging with vinegar and water is had recourse to; saline antimonials, or simple effervescing draughts, are (more especially in private practice) occasionally administered; wine, and acidulated bitters, under proper modifications, in the advanced stage of the disease, not unfrequently prove beneficial. The sick diet is tea or water-gruel at breakfast; rice and milk, bread panada, and, in some cases, weak beef-tea to dinner, and thin gruel for supper. When the tongue, though clean, appears dry after the action of repeated cathartics; or where sordes collects upon the teeth and gums; or when the stomach seems oppressed with flatulence, and in a state of atony, I have seen a spoonful of yeast, to which a little aromatic is added, given three or four times a-day, prove serviceable. Pulmonic and other anomalous symptoms are treated as the indications may direct. Such is an outline of the practice I pursue; and it has hitherto answered my expectations in the treatment of the prevailing epidemic. Relapses have been more frequent in private than in hospital practice.

An unusual number of our cases were women giving suck; we have had them from three weeks or a fortnight after delivery till the fifteenth month. The primary symptoms were frequently severe; and the pain and tension of the mammæ appeared to lengthen this stage, but when the distention subsided, the patient soon did well. Brisk cathartics and emetics seemed to dispel the milk; a more steady use of these, with occasional lubricating embrocations, were all the extra measures had recourse to in perfecting the cure. Abscesses did not occur in a single case, though, from the stony hardness of the breasts for the first few days, I was prepared to expect them.

The numerous discussions which have lately taken place respecting the treatment of fever, would naturally lead one unacquainted with the subject to the conclusion that the ancients, as well as our immediate ancestors, and even the senior practitioners of the present day, had done little besides leaving nature to herself, or poured in wine, bark, and brandy. He who shall take the trouble, however, to peruse and attentively consider the writings of Hippocrates, Celsus, and Aretaeus, with a few other of the ancients; of Boerhaave, Stahl, Haller, and Hoffman; and more especially the doctrines laid down by the illustrious Sydenham, and the no less eminent Cullen, with some other original writers, will find that most of the means now in use, most of the inductions so pompously detailed, and most of the conclusions drawn from these inductions, were little less accurately understood and acted upon, though under the influence of different pathological views, than we now find in the present day. Nor is it fair, I conceive, as is too often done, to blame the doctrines laid down, or treat with contempt the precepts inculcated in the schools. No medical teacher can do more than deliver his own opinions, and point out the sources whence these opinions were formed. Sydenham has deservedly been in the mouth of almost every medical professor, and whose is the fault if he is not studied?

Bleeding in fever has either directly or indirectly been claimed as a discovery by some scores of the profession, within the last twenty years. Calomel, opium, blisters, emetics, cold affusion, sponging, &c. have all come in for a share of the same honour; even green tea is magnified into a potent remedy; one theory knocks down another; hypothesis succeeds hypothesis, as wave succeeds wave. In this conflict of opinion, that practitioner stands, I conceive, the best chance of success, who calmly and deliberately investigates and prescribes for symptoms as they present themselves, without regard to theory, from whatever authority promulgated. From not attending to this circumstance, our rationale of prescription is often clouded and indefinite, if not occasionally absurd. For, in the very subject of this paper, I am more and more convinced, that much irremediable mischief has been done, and is daily occasioned, by treating typhus in the beginning in reference to the appearance it is expected to assume, and which, if neglected, it will assume in the latter stage. If, by energetic measures, the fever is checked in the first stage, we find little or no consequent debility; but if allowed to proceed, the previous damage will, in many instances, render all our future efforts vain and nugatory. It is cheering, however, to reflect, that this opinion is daily gaining ground, and, though not an idea of yesterday, the medical public are much indebted to Drs Clutterbuck, Mills, Jackson, Armstrong, Bateman, and others, for placing it on such prominent ground.

Although I have much to see and much to learn, ere I dare decidedly venture to oppose my judgment to the two last mentioned eminent writers, still, as I wish to make experience my guide, I may observe, that the method of cure recommended by Dr Armstrong, viz. by salivation, is more severe than the fever, as it has hitherto appeared to me, seems to require; but, even admitting that it were the most certain method of cure, which I am very much inclined to doubt, salivation, with whatever care produced, gives a shock to the system, which is not easily got the better of; and the combined influence of the fever and mercury calls into activity any latent pulmonic or scrofulous affections, which is often attended with deleterious consequences. Besides, it is well known, that, except bleeding is premised to lower the symptoms, and is of itself frequently sufficient to check the fever, the patient may proceed to the very gates of eternity, ere the calomel and opium shall be able to overtake him; and when they do, it is uncertain whether they shall then be able to prevent his entrance.* Admiring as much as any one the spirit of his writings, and the many new and striking ideas in his pathological deductions, I find still greater fault with his nosological arrangements. For, as this department of medicine must ever, in some measure, be arbitrary, by an unnecessary multiplication of terms, and by departing from those received by universal consent, he has tended, in my opinion, to make "confusion worse confused," without, in any one important instance, rendering our views of the subject more explicit. His division of typhus is particularly exceptionable, as in one single case we frequently find the prominent symptoms of all his varieties. This is sufficiently apparent, from his being perpetually obliged to refer to the other two, in the treatment of the third. Indeed, his inflammatory and congestive typhus are evidently two stages, and not varieties of the same disease, comprehended with greater accuracy, and at least equal propriety, under the Synochus of Cullen. His simple typhus is, to all intents and purposes, the typhus of the same eminent nosologist. For the same reasons, I deprecate the arrangement of Dr Mills being introduced into practice. Certain ideas are too frequently connected with nosological terms, and enteritic or hepatic fever may, with many practitioners, be associated with the hepatitis or enteritis of Cullen, with the most fatal result to the patient labouring under the latter diseases. When we consider that the most accurate delineators of fever have in every age described it so much alike, and take along with us the powerful influence of climate and temperament, the different manner of living, and a thousand other agencies which act upon the human constitution, all tending to produce symptoms, the majority of which are present in every species of fever, it might induce us to suppose that they are only varieties of one great whole, differing solely in degree from the most malignant plague of Tripoli, to the mildest typhus of England.

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* Since this paper was wrote, the Clinical Reports of the Edinburgh Royal Infirmary have been put into my hands. I have only yet been able to glance them hastily over. As may be expected, however, considering the source whence they spring, they seem to contain much valuable and interesting information. I am peculiarly gratified to find the ideas of the salivating practice contained in them, so consonant to my own. But as no practice recommended from such authority should be lightly condemned, further experience will enable us to draw more certain conclusions. I may here be allowed to suggest the propriety of a skeleton table being drawn up, and inserted in the next Journal, shewing the best method of noting the different particulars required in Fever Hospitals, and another for General Hospitals. These might be brought into general use in 1820; and we would thus obtain an extent and uniformity of results, which, in the present state of reports, we in vain look for.

The judgment of the faculty has already deservedly stamped the character of Dr Bateman's treatise. By his verdict, and those of other contemporary writers, bark seems now struggling at the last gasp for medical existence in fever; and although it is cruel to insult a fallen enemy, I cannot forbear considering it as such in the present epidemic, having seen it in a few instances I think, do harm. Yet the names of Morton, Pringle, Huxham, Fothergill, Lettsom, Sims, and a host of dead and living authorities, shed a lustre over this once highly valued medicine, and ought rather to make us view it with distrust, than absolutely reject it from practice, for doing which we may be laughed at by a succeeding generation. The experience of ages shows,—the records of hospitals show,—the very tables which I now send you, in which it has been in pretty general requisition, declare, that it is not such a noxious deadly vegetable as some recent writers would make us believe; and where a vegetable tonic is deemed necessary, I do think it may be used, if not in preference, as well as any other. If it has a specific febrifuge effect, which I am not at present prepared either to confirm or deny, its *modus operandi* appears to me not unsimilar to mercury in the venereal disease, which, inducing a fever *sui generis*, overpowers the other. This, where congestion is present, must be dangerous; and where the fever is mild, is uncalled for. It seems to affect the pulmonary system more than any other; and the principal danger, after all, may arise from its too early, or otherwise improper exhibition.

That we possess many more powerful remedies in fever than antimony, is indisputable; but as an expectorant, or where fever is treated from the beginning by sweating, as is still done by some practitioners of the " olden times," or where we wish a diaphoresis to be produced, it not unfrequently proves useful, and ought therefore to be treated with some respect; for if, from the scepticism of some, and unskilful management of others, we go on, blowing up medicine after medicine, on partial reports, we shall soon have none left but the surgeon's steel and cold water. Dr Bateman seems also to esteem the power of local blood-letting in a much lower scale than general experience has rated it; and it is curious to observe the very different result in both, drawn from experience, given on this subject by him and Dr Armstrong. With these exceptions, I do not know a single treatise more worthy of perusal by the practitioner who wishes to treat typhus fever in a rational and judicious manner.

In regard to the fever in the other parts of Yorkshire, I cannot give you much information; but it has undoubtedly been much more prevalent than in former years. My intelligent friend Dr Mossman of Bradford informed me, some time ago, that he visited 27 cases at the same time in one village; and I trust this gentleman, who is so capable of observing and describing, will in due time give an account of them to the public. When at Halifax, in the summer, typhus had increased so much, that, as Dr Moulson informed me, they were about to establish fever wards in addition to the Dispensary of the town. This prudent measure has since, I believe, been carried into effect. Dr Moulson has, I understand, been very successful in his treatment, by abstracting blood from the temporal artery. In the highlands of Yorkshire, in a village near Settle, I have heard that the fever has proved alarmingly fatal.

Should my information prove correct, and this meet the eye of any practitioner in those parts, it is to be wished he would transmit you an account of it; for it is principally by observing the progress of fever in remote villages, where the intercourse is limited, that we can accurately ascertain the influence of contagion. Had the venerable Dr Haygarth commenced his career in London instead of Chester, we should never have obtained those important results which constitute our chief knowledge on this subject. A medical gentleman from Ripon informed me yesterday, that it was also prevalent in that quarter; and, from similar anthority, it has been no less frequent in Huddersfield and Wakefield. In Atley, a small sequestered town ten miles from this, the fever has lately made its appearance. The infection was supposed to be brought from Leeds In all these places, as well as Leeds, it has mostly been confined to the lower orders, and has not generally proved very fatal.

Leeds, 27*th Nov.* 1818.

XI.

Case of Rupture of the Liver, with Inflammation and Suppuration, proceeding from external violence By ROBERT HENRY BELL, Assistant Surgeon 86th Regiment, in immediate charge of Flank Companies on Field Service in the Palnaud.

THOMAS BRIGGS, a corporal in the light company of His Majesty's 86th, or Royal County Down Regiment, was about 27 years of age, well made, compact and active, of sober habits, pale complexion, light hair, and blue eyes. On the night of the 2d of February 1818, he applied at the hospital tent for something to relieve an incessant vomiting; his pulse slow and small; extremities cold, and skin covered with a clammy sweat. Fomentations to the abdomen and pediluvium restored heat to his limbs, and a blister over his stomach, with ten grains of calomel, and four grains of opium, at length allayed the constant vomiting.

3d February 1818.—This morning gave the following account of himself: "He said, that, early on the morning of the 1st instant, before daylight, when running out of his tent, something tripped his foot. He staggered several steps forward, in the vain attempt to recover himself, when he fell with the whole weight and force of his body, his stomach striking against a tent-peg of an opposite tent. The shock at first was very great; he felt sick and faintish, and vomited a good deal; but as he did not wish to report himself sick, as the detachment was in active duty against Pindarrees, he endeavoured to recover himself, and marched with his company two long marches, though he had a constant pain at the lower part of chest, and a sense of dragging weight at his stomach. After the second march, when he arrived at the ground of encampment, he felt very sick, and vomited up his breakfast, and every thing he swallowed. *Burned spirits*, as a *certain specific*, were administered to him by a comrade; but as he vomited this up likewise, he then applied for relief at the hospital tent." Such was his own account of the cause of his present complaints.

Blister rose well, and discharged very freely. Pain has moved from the stomach to right side; pulse 96, hard and frequent; has had no stool for the last two days; tongue furred.

> Detrahe e brachio dextro sanguinem ad uncias sexdecim. Habeat statim sulph. sodæ ℥i. et enema purgans etiam si opus fuerit.

4th.—Twelve ounces of blood only were taken from his arm, as syncope was then induced. Had vomited three successive doses of salts, and used two purging clysters ere his bowels were moved. Passed several dark fetid stools, and feels much easier and lighter since the evacuation. Pulse 88, soft and fuller; tongue white; skin warm and dry.

> ℞ Calomelan. gr. xii. Pulv. antimon. gr. xviii. M. Fiant pilulæ sex æquales. Sumat unam omni secundâ horâ.

5th.—Has now no pain, even on pressure over the region of the stomach, and observes that he could cover with his thumb the extent of the pain he feels in his right side. Complains of his want of appetite; and that he has not been able to use any food for several days, as he has had no inclination for it. Tongue white; pulse 90, but soft and full.

> Pergat in usu pilular. cal. et pulv. antim. No. 6. Applicentur hirudines duodecim lateri dextro statim.

6th.—Leeches bled well, and removed the pain from his right side. Says that the pain is completely moved from his right side into his right loin, and puts his hand just over the region of the kidney, as the part he feels the most pain in. The urine he passed about an hour ago is still highly red, though it has deposited a very reddish sediment.

Pergat in usu pil. cal. et pulv. antim. No. 6. Hab. calomel gr. x. hora somni, et cras mane sulph. sodæ ℥i.

7th.—Passed many dark-coloured fetid stools after the salts. Pain in his loins somewhat better. Urine sometimes quite clear, at other times (in the same hour) like the muddy grounds of beer. Tongue white; pulse 94, but full and softish.

Cont. medicamenta ut heri præscripta. Utatur fomentatione regione lumborum.

8th.—Had several hard fetid stools after the calomel and salts. Feels easier and better. Pulse still a little hard and quick, (94.) The pain in loins continues. Describes it as shooting down from right loin along his back bone. Has no numbness in thigh or leg of right side, nor is the right testicle retracted. Says he feels very weak and languid. Tongue moist, but white. Lips dry; always feels thirsty.

Cont. pil. cal. et antim. No. 6. Applicentur hirudines decem parti dolenti lumborum. Habt. potum acidi nitrici diluti ad Oct. ij. quotidie.

9th.—Leeches bled his right loin very freely, but does not feel himself in the least better or easier. On pressing upon the right side, complains of a dull sensation of pain there. Tongue still white, but moist; pulse 96, and hard, but not tense.

Habt. pil. cal. et antim. No. 8. Applicetur lateri dextro emplast. lyttæ largum.

10th.—Blister rose well; thinks himself somewhat better since. Urine still occasionally turbid. Had some stools of a yellowish colour, and more natural consistence. Tongue still, however, continues whitish, though moist. Pulse 92, but has a hardish, unpleasant throb. Gums are pale, but not swelled. Ptyalism not in the least degree induced. As he complained of weakness, and flatulence in stomach, directed four ounces of Madeira wine to be mixed in sweetened congee (rice gruel) according to his taste, daily.

Omittr. pil. cal. et antim.

℞ Calomelan. ℈i. Opii puriss. gr. iiss. Fiant pilulæ decem. Sumat in die.

Fricat femoribus ung. hyd. fortiss. camph. ʒij.

11th.—Pain at loins at present most troublesome. Does not think that the blister has relieved the dull pain he felt in his right side. Tongue moist, but white; pulse 92, and gives a hardish throb; skin dry, but not to his own sensations hot.

Cont. medicamenta ut antea. Habt. haust. anodyn. hora somni.

12th.—Pain in the side and loins more severe. Says he feels himself daily getting weaker and worse. Stools continue of a natural colour and consistence. Urine high-coloured, but not turbid; tongue white; pulse as yesterday.

Cont. pil. cal. et opii No. 12. et ung. hyd. fortiss. ʒij. Applicentur hirudines duodecim regioni lumborum.

13th.—As yesterday in every respect, except that the pain in loins is much better since the leeches bled him.

Pergat in usu medicam. Hab. vin. Madeir. ℥vi.

Vespere.—Pain in side has become suddenly more severe, striking down along his back-bone. Says he cannot draw a deep sigh, or breathe without it hurting him, nor can he now lie down with ease or freedom. No stool since morning. Pulse 98, hard; tongue white.

Applicentur hirudines duodecim statim, et postea emplast. lyttæ largum parti dolenti. Sumat calomel gr. x. statim, et cras mane sulph. sodæ ℥i.

14th.—Blister rose well, and physic purged him freely. These, with the leeching, somewhat relieved the pain in side. The first stools after the physic were dark, the latter naturally coloured. Urine reddish; gums pale and flaccid; pulse 94, hardish; tongue white.

Habt. pilul. calom. et opii No. 20. et ung. hyd. ʒij. Sumat. vini Madeirensis ℥viij. quotidie.

15th.—Thinks himself, on the whole, rather better to-day. Feels weakened, in consequence of a considerable discharge from blistered surface.

Pergat in usu medicamentorum ut heri.

16th.—Not just so well as yesterday. Ptyalism not in the slightest decree induced. Gums pale. Has now no perceptible pain in side or loins, even on pressure, except on the blistered surface.

Pergat in omnibus ut antea.

17th.—No better. Pulse 90, softer and fuller than for some time past. Tongue still white. Skin cool, and not dry. No ptyalism. Gums pale; and no pain in side or loins.

Cont. medicamenta ut antea. Habt. sulph. sodæ ℥i. cras mane.

18th.—Salts have not yet operated. Has taken a second dose. Feels hot, and appears quite languid Says he feels very CURIOUS, as he has no pain but a listlessness and carelessness about any thing around him, and that he is very weak.

Cont. medicamenta ut antea.

19th.—Salts purged him freely Feels himself much livelier, and consequently better to-day. Belly appears tumid. Swelling pretty generally diffused, but perceptibly greater about the stomach and lower part of the chest. Swelling soft to the touch, not elastic; nor is there any pain on pressing over any part of the surface. Pulse 86, and soft, compressible.

Cont. medicamenta ut antea.

℞ Ung. hyd. fortiss. ʒiij. Ol. olivæ ℥i. Aq. ammoniæ ʒij. Tere simul. ut fiat liniment. toto abdomini perfricandum.

20th.—Passed a very restless night, and appears evidently much worse. Says he feels the swelling in belly to greatly impede his breathing. Feels his stomach as if drawn down, as it were, by a weight, and has considerable oppression about the precordia. Pulse 78.

Omittr. pil. cal. et opii. Pergat in usu vini Madeirensis ℥viij. Pot. acid. Ung. hyd. fortiss et Liniment hyd. ammon. Habt. statim. sulph. sodæ ℥i.

℞ Calom. ℈ii. Pulv. Ipecac. ℈i. Ol. menth. Mma 6. F. Pil. 20 æquales. Sumat unam omni hora.

Vespere.—Salts purged him very freely, but did not find himself any better after them. At 5 o'clock P M. he felt an inclination to go to stool, when he passed, at two motions, nearly SEVEN PINTS of a sanious matter, like blood and water mixed, and without any fetor; he likewise passed five balls, round, semitransparent, one inch and a half in diameter, elastic, compressible, but regaining their rounded appearance when the pressure was removed. When cut into, they exhibited the same appearance throughout, and were exceedingly like very strong highly red jelly, or soft glue. There was no appearance of concentric circles or layers. They sank slowly in water.

℞ Spiritus vini (Anglice *brandy*) ℥vi. Sacchari purific. ʒvi. Aq. Calidæ Oct. j. M. Fiat. pot. cardiac. pro nata sumend.

Hab. quoque vini Lusitaniæ ℥iv quotidie.

Circumcingatur abdomen laneo panno longo, latit. quatuor digitorum.

21st.—Passed a very uneasy and restless night, although he had two anodyne draughts. Had no stools since 9 o'clock P.M. Says he feels no pain. Appears extremely anxious. Breathing oppressed, and drawn with sighing. Tongue dry and red; lips very dry; pulse 80, and small, but compressible; has had no rigor, nor does he recollect of having had any since he took sick.

Con. medicamenta ut antea. Habeat iterum sulph. sodæ ℥i.

22d.—Passed an almost sleepless night. Salts purged him gently, and caused him to pass many balls of the elastic jelly-like matter he parted with on the evening of the 20th, as well as some lumps of hardened feces. The swelling of the belly is more increased, particularly so over the site of the arch of the colon. Abdomen feels soft on pressure, and somewhat elastic. Tongue moist at the fore part, but black at the root; lips dry; gums swelled, but no ptyalism; pulse 76.

> Cont. medicamenta ut antea. Habeat sulph. sodæ ℥ss. statim.

23d.—Had several greenish and dark stools after the salts. Passed no more blood or lumps. The stool he had just now is perfectly natural in colour and smell, as well as consistence. Tongue dry and clean; gums pale and flaccid; teeth black; vesicles containing pus on thighs and parts where inunction has been used; pulse 64, slow and feeble.

> Omitt. pil. cal. et ipec. et ung. hyd. fortiss. Cont. alia. Utatur potû acidi nitrici dilut. ad libitum.

24th.—Evidently moribund. Occasionally delirious, and at intervals. Gums, tongue, and lips, perfectly dry; tongue and teeth black; pulse intermits every tenth beat. At 6 o'clock P.M. pulse at wrist could not be felt at all. Breathed until 9 o'clock P.M.

Extispicium cadaveris.

Externally.—The abdomen, tumid and elastic over the site of the arch of the colon, exhibited no wound, bruise, or any other mark, except where the leeches and blisters had been applied. The integuments every where separated easily from the muscular substance of the belly; but the adhesion of the peritonæum with the transversalis and inner sheath of the recti was so strong, that the knife was required in most parts to separate them.

Internally.—The peritonæum, very thick and pale, had no appearance of having suffered from recent inflammation. Upon cutting into it, three pints of yellowish-coloured clear serum like whey were lifted from among the viscera. The epiploon, devoid of fat, lay loose over the intestines. The stomach, without food, pale. with thickened coats, and very greatly distended by flatus, was pushed (as it were) down to the umbilicus, and had formed that appearance, which, before the abdomen was opened, was conceived to be the colon tumid with flatus. It had no marks of very recent inflammation, inside or outside. The small intestines were perfectly natural, free from adhesions, or any mark of recent or former disease. The rectum, bladder, and left arch of the colon, were also perfectly sound, inside and outside. The right arch of the colon adhered to the liver and peritonæum, covering the loins by its outer coats only, as the inside coats had not the slightest mark of disease. The caput cæcum coli was in every part, except one, perfectly sound and free from inflammation, and this spot was on that part which is connected to the loins. The ulcerated part was about an inch in diameter, and a sinus was discovered leading up behind the peritonæum, from this place towards the right lobe of the liver, which was next examined. It projected about three inches beyond the cartilages of the ribs. A considerable ragged cleft, covered by a thin vascular membranous substance, appeared on the anterior inferior edge of the right lobe, about an inch from the fundus of the gall-bladder;—adhesion had joined the more contiguous parts, as in cases of lacerated wounds of the integuments.

The convex part of the liver did not adhere to the diaphragm; but upon cutting into a whitish spot on the more anterior part of the convexity of the liver, an abscess was cut into, which contained two and a half pints (by measure) of well concocted good pus. This abscess had no connection with any other. Passing the hand round the liver, so as to raise its right lobe, adhesions to the lesser diaphragm and peritonæum, covering the loins, were found, which, in endeavouring to separate, a great abscess was burst into. This contained (by measure) four pints of good pus, a little thinner than that in the upper one. A thick bougie passed easily from this abscess along the convex edge of the right kidney, behind the peritonæum, into the ulcerated part of the cæcum. The right kidney was somewhat enlarged, but no disease of structure. The left kidney was perfectly sound, and the ureters of both pervious and natural. The gall-bladder was completely empty, but its ducts perfectly free. The left lobe of the liver, the lobulus Spigelii, and such of the right lobe as formed not any of the paries of the abscesses, was of a natural colour, and of sound structure. The pancreas was large, hard, and much lobulated, but not gritty when cut into. Mesenteric glands free from disease. On opening the chest, the lungs were found perfectly healthy; no adhesions. The pericardium, however, contained more than a pint of liquor, of a yellowish colour; and the heart was large, pale, and flabby.

In all cases of hepatitis which I have seen, (and hepatic diseases of all kinds are peculiarly rife in this part of the coast), I have never witnessed a case which originated from external injury solely, or which ran so rapidly from inflammation into suppuration, without exhibiting in its progress some more unequivocal symptoms than this case presented. Here there was no great intestinal derangement indeed, except in one or two days; the egesta were perfectly natural; no CONTINUED pain or tenderness to the touch in the region of the liver; no sympathetic pain in the shoulder or clavicle; no rigor; tongue did not become *red* and *dry* till the 21st day. In every case of abscess in the liver which I have examined, I have never seen so pure pus, unmixed either with particles of dissolved blood or bile. The sponged paries of the abscesses had every appearance of the bottom of a healthy ulcer; and in no part of the liver (except where the abscesses were) was there the slightest appearance of structural derangement. This last circumstance is very unusual in all cases of hepatitis, on the Coromandel coast at least.

Camp at Datchapilly, near the Banks of the
Kistna, E. Indies, 12th March 1818.

XII.

Cases of Severe Dyspnœa in Children, connected with a Singular Affection of the Throat. By JOHN ABERCROMBIE, M. D.

I HAVE met with three cases of violent difficulty of breathing in children, connected with an affection of the throat which was new to me, and perhaps may be not unworthy of a corner in your Journal.

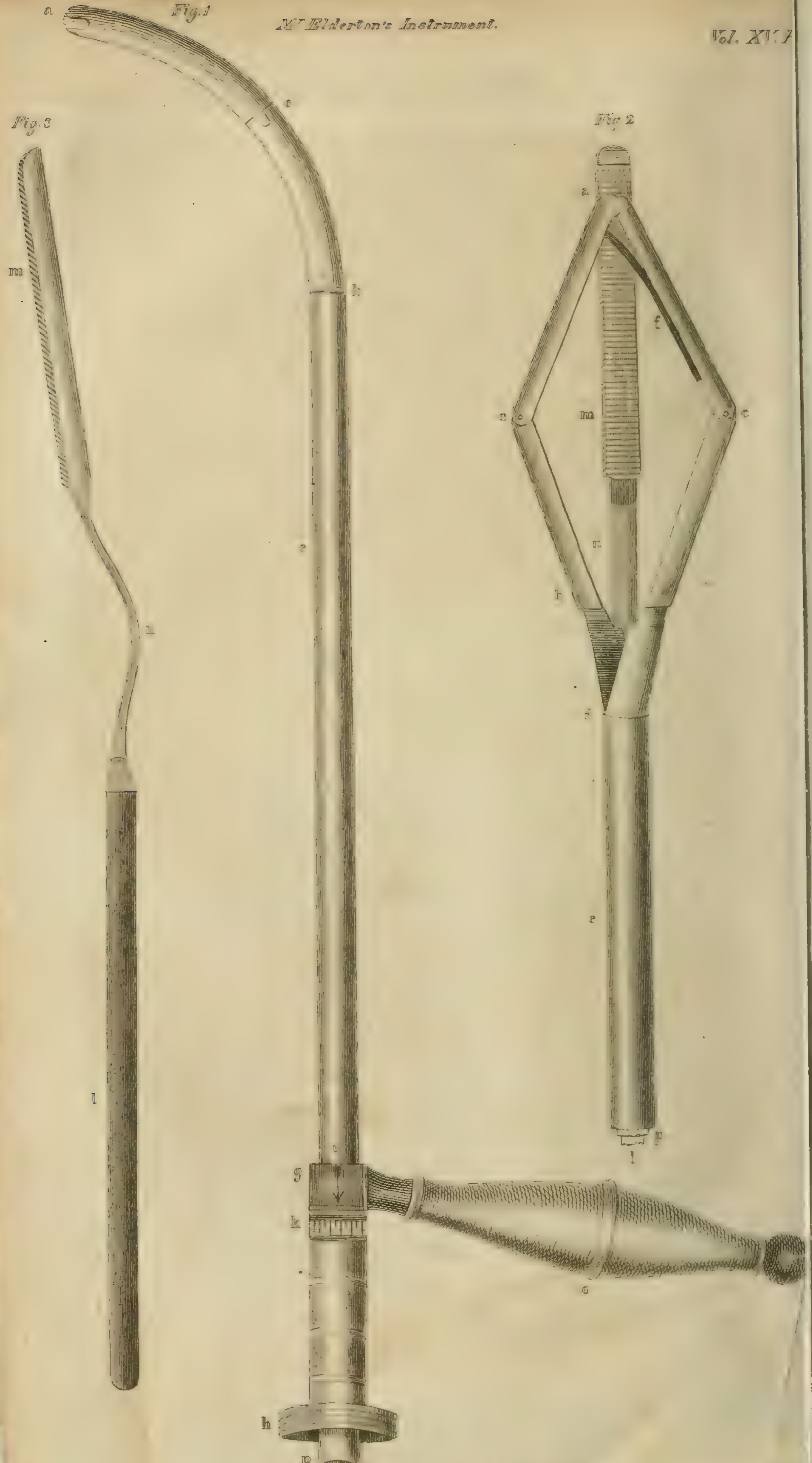
CASE I.—Was a child about two years of age. I found him affected with severe difficulty of breathing, resembling the last stage of croup, inspiration being particularly laborious, and performed with a strong exertion of all the muscles connected with it, and a sound which conveyed the impression of great interruption to the passage of air through the larynx. At first sight, I considered the disease as croup; but on inquiring into the history of it, I was told that the affection had existed in nearly the same degree of violence for eight days; and that the health of the child had not been in other respects remarkably impaired. Concluding from these circumstances that the disease could not be croup, I was led to a particular examination of the throat, where I found the tonsils and other superior parts quite healthy; but in the lower and posterior part of the pharynx I observed an appearance of fulness, which, however, was situated too far down to be seen distinctly. On examining this part with the finger, I discovered a tumour, arising from the posterior part of the pharynx, and filling up the space completely, so as to press considerably upon the larynx;—the tumour was soft and fluctuating, and evidently contained a fluid. With considerable difficulty I succeeded in puncturing it, by directing a lancet along the fore finger of the left hand, which was kept in contact with the tumour, and purulent matter was discharged from it, with immediate and complete relief of all the symptoms. In the course of the following night, however, the dyspnœa returned; and on examining the throat next day, I found that the tumour was again filled with matter. It was therefore necessary to puncture it again, by which the dyspnœa was again completely relieved. In this manner it required to be punctured four or five times, after which the case terminated favourably.

Case II.—Was a child aged eighteen months, who had passed through measles eight or ten days before I saw her. Her breathing had first become difficult during the eruption of measles, and after partial relief, procured by the usual treatment, had again become difficult after the decline of the eruption. The difficulty had been gradually increasing, particularly during the last three days, and in the night before I saw her, had been so great as frequently to threaten immediate suffocation. On examining the throat, I discovered a tumour which exactly resembled that described in the former case, except that it was situated rather lower down, so that it could only be felt by the finger, and nothing remarkable could be detected by looking into the throat. With some difficulty the tumour was punctured, by means of a small trocar, such as is used for hydrocele, the canula being first carried down so as to bring the point of it into contact with the tumour, and then the stilette introduced through it. There was a copious discharge of purulent matter, with immediate relief of the symptoms; and I requested an intelligent young medical man, who was related to the family, to make pressure on the tumour several times in the day, so as to prevent it from filling again, as in the former case. By this precaution there was no return of the symptoms. A remarkable circumstance in this case was, that the child appeared to have little or no difficulty in swallowing.

Case III.—Was so exactly similar to the preceding, that it is unnecessary to describe it particularly. The tumour required to be punctured a second time, after which the case terminated favourably.

Though not immediately connected with that subject, I take the opportunity of adding a very few observations on the larynx as a seat of disease. The more violent diseases of this organ have been investigated with much attention; but I think there are more obscure affections of it, which present a most important field for observation, in which, hitherto, little has been

Mr Elderton's Instrument.



Published by A. Constable & Co. Edinr 1st April 1819.

done. Many obstinate cases of *cough* probably have their seat in the larynx, which we are sometimes disposed to refer to the lungs. I lately saw a gentleman, about 50 years of age, who was affected with the most violent cough I ever met with. It occurred with greatest violence in the night time, when it frequently kept him for an hour or two at a time in one continued paroxysm of the most violent convulsive coughing; his features became turgid; he nearly lost his recollection; and there seemed every reason to fear that the paroxysm would terminate either by suffocation or apoplexy. The disease had continued about a fortnight, and the most active remedies, large and repeated blood-letting, blistering, opiates, and antispasmodics, had not made the slightest impression upon it. His pulse and breathing were natural; he had a peculiar hoarseness or huskiness of his voice; and, upon inquiry, he mentioned that the paroxysms were preceded by a particular sense of irritation about the larynx. The application of six leeches to the region of the larynx broke the force of this frightful disease, and a second application of the same remedy completely removed it.

It may be worthy of investigation, whether hooping-cough has any relation to the larynx. Some cases also, which have been considered as consumptive, have afterwards turned out to be connected with disease in that organ; and, upon the whole, there seems to be much room for extensive observation, with the view of introducing precision and accuracy into the pathology of cough.

XIII.

Description of an Instrument for destroying Urinary Calculi within the Bladder; with Remarks on the practicability of its employment in the living subject, and the probability of success. By Mr John Elderton, General Infirmary, Northampton *(With an Engraving.)*

There is now before me a model of the instrument I am about to describe, which, as a whole, bears a strong resemblance to a common, full-sized catheter, straight as far as the curve. The annexed representation will convey a pretty accurate idea of its form, both in the front and lateral view.

That part which forms the curve, (a,b, fig. 1.) consists of two blades, (c, c, fig. 2.) placed laterally to each other, and which, when in contact, present a solid, round form, (c, fig. 1.) about the size of a large bougie. At that extremity of the curve answering to the point of the catheter, (a,) the blades are connected by a joint; one blade being received within the other, and presenting this united and uniform smooth point. At an equal distance, (c, fig. 1.) between the commencement of the curve (b,) and its extremity, (a,) a second joint connects the other half (c, b,) of the same side. This structure is common to both the right and left blade. At the commencement of the curve, (b, there is another joint, (d,) uniting the two blades, which are here fixed to a hollow metallic tube, made to slide readily within an outer canula, (b, g, fig. 1.) A small steel spring, (f, fig. 2.) placed within the blades, at their extreme point, expands them, and they then present an opening of an ovate shape, (a, c, b, c, fig. 2.) The blades may be regularly and firmly closed, by simply drawing the inner tube within the outer one, while, by pushing down the second tube, the blades may be expanded as before. When thus expanded, they constitute a pair of forceps, (c,c, fig. 2.) well adapted, from their shape, capacity, and strength with which the blades may be closed, to grasp, and hold firmly, a moderately-sized calculus.

To the top of the tube (g, fig. 1. corresponding to that part of the catheter which is grasped by the hand while introducing it) there is attached a moveable apparatus, having a spiral groove on the inside, made to receiv a small stud fixed into the second (inner) tube. By turning the above apparatus backwards or forwards with the finger and thumb, the second tube is made to slide upwards or downwards within the outer canula. When the inner tube slides upwards, the blades are of necessity closed, (fig. 1.) by being partially withdrawn within the outer canula, which thus compresses and confines them. When, on the other hand, the second tube is caused to slide downwards, the blades are necessarily protruded, and, being no longer confined by the outer tube, they are expanded into the ovate shape, (a, c, d, fig 2.) mentioned above, by means of the spring (f, fig. 2.) formerly described, at their point; so that, with the assistance of a mark on the outer tube, (i, fig. 1.) and an even division by lines (k, fig. 1.) of the circumference of the apparatus, (k,) which turns, the diameter (c, c, fig. 2.) of the expanded blades within the bladder is ascertained, and, consequently, the size of the calculus grasped by them, upon which we have to operate. Thus, then, we have an instrument, shaped like a catheter, perfectly smooth throughout its whole surface, readily passing through a healthy adult urethra into the bladder, where, by turning the screw-apparatus above described, the curve expands, and forms the forceps, the capacity of the expansion of which will be known by referring to the index-lines marked on the top of the instrument.

The only remaining part of the instrument to be spoken of is the *rasp* for filing down the calculus supposed to be secured in the bladder, by means of the forceps shown above. There will be no difficulty in understanding this part of the instrument. To a steel rod, (l, fig. 3.) the length of the whole instrument, is screwed a rasp, (m,) supported on a curved spring, (n.) The use of this spring is to keep the rasp in constant contact with, and pressure upon the stone; which, how much soever diminished by friction, will still, from this method, have the rasp acting firmly upon its under surface. The elasticity of the spring admits of its being brought into a right line with the rod, and thus allows, without any impediment, the rasp to pass down the second tube, and to come out between the blades of the forceps, (fig. 2.) and *beneath* the stone, to which it is directly applied by the spring on which it is supported. All that need be said as to the principle of the instrument's operation is, that we retain it firmly with the left hand, by means of a metallic handle, (o, fig. 1.) and work the rasp with the right.

I will farther add, that as the second tube will only allow of a rasp to pass inferior to its own diameter, it is evident the section made on the stone will be such as to leave two lateral portions untouched; but these portions may be also filed down, by merely having two other rasps, whose springs shoot out laterally, so as to expose the untouched surfaces to the action of the instrument; at the same time, these rasps, being well tempered, will admit of being brought into a right line, like the central rasp before described. For convenience, I have also had the rod on which the spring with the rasp is screwed, made small enough to slide within a tube, into which, by means of a similar apparatus to that affixed to the outer tube, the spring may be more readily caused to form a right line, than if it were pushed through with the hand; besides, this contrivance presents the advantage of regulating the distance to which it may be desirable for the rasp to act within the bladder.

Remarks.—The first objection likely to be urged against this instrument is the complexity of its structure; but this complexity is rather apparent than real;—for the mechanical principle on which its different parts act is sufficiently simple. From a model, or even from an intelligible diagram, it could be made by an artist of moderate skill in any part of the kingdom; and when once made of good steel, it is not apt to become out of order, or to want repair. The whole apparatus may be comprised within the length and thickness of a large-sized catheter. Its large size, instead of operating as an objection, will facilitate its introduction, as its point will not catch the folds or lacunæ of the urethra, which a small instrument would be apt to do.

The difficulty of seizing the stone with these forceps may be started as another objection to the instrument; but this difficulty will not occur, I apprehend, to an operator of moderate dexterity. The instrument should never be used except when the bladder is pretty considerably distended with urine. The patient should be placed in the recumbent posture, and the instrument used as a sound to ascertain the situation of the stone; which, under these circumstances, will be generally found in the posterior part of the bladder, nigh the fundus. Having felt it with the instrument, the blades are to be expanded by the spiral screw at the top of the instrument, and when we have laid hold of the stone, it is to be firmly secured, by turning the screw in the opposite direction. The diameter of the body grasped by the blades may be ascertained by the index before mentioned.

The stone being thus laid hold of, the operator is now to introduce the rasp along the cavity of the second tube; but before beginning to work it to and fro, for the purpose of grinding down the stone, it will be prudent to take the precaution of bringing the concave part of the instrument close up under the arch of the pubes; it should be retained steadily in that situation, (in the same manner as the staff in the common operation of lithotomy,) in order to prevent all risk of hurting the posterior part of the bladder by the successive strokes of the rasp.

It will scarcely be objected that the teeth of the file are likely to be clogged up, and made inefficient by the dust of the calculus; in as much as the body of urine in the bladder will at each stroke wash the filings away. But if this were not found to be the case, the rasp might be made with little holes, like a nutmeg-grater, by which means the dust will pass through.

Upon the whole, I conceive the ultimate operation of the instrument will be, to reduce the calculus to such a state of comminution, that the sand, and even the minute portions that remain after the successive applications of the file, may be voided through the urethra along with the urine.

I am willing to flatter myself, that such members of the profession, as may have the opportunity, will give this instrument a fair trial.

February 4th 1819.

PART II.

CRITICAL ANALYSIS.

I.

Practical Illustrations of the Scarlet Fever, Measles, Pulmonary Consumption, and Chronic Diseases, with Remarks on Sulphureous Waters, &c. By JOHN ARMSTRONG, M. D. 8vo, pp. 448. London, 1818.

We have the highest opinion of Dr Armstrong's talents, and of the practical value of his writings. His admirable work on Typhus—now stamped by the broad seal of public favour,—has, doubtless, contributed very essentially to diffuse those improved and enlightened views of the pathology of fever, which have gradually been obtaining for a number of years past, but which, until very lately, have been confined to the higher and better informed classes of the profession : indeed, we have observed with pleasure that there is scarcely a practitioner, even in our most sequestered villages, who has not read Dr Armstrong, or who does not profess to act upon his maxims! When an author of such judgment becomes popular, the circumstance is as creditable to the discernment of the great body of the profession, as it must be useful to the well-being of the public at large.

His present work is certainly not equal to the former one ; yet it contains a great deal of information which, both by its value and its novelty, will repay the trouble of a diligent perusal. We are at no loss to discover in it traces of the same master hand,—of the same talent for observation,—the same fidelity and liveliness in the delineation of disease,—the same ingenuous modesty in the reprobation of old, or the exposition of new doctrines and practices ; yet, upon the whole, the general spirit of its execution is more diffuse and desultory ; and it has not that precision of remark and air of maturity about it, which testified, by internal evidence, that the treatise on typhus was the product of time and reflection.

The work opens with a very excellent account of the history and treatment of Scarlatina, which he divides, as usual, into three forms; viz. 1st, Scarlatina simplex; 2d, Scarlatina anginosa; and, 3d, Scarlatina maligna. The first two kinds we dismiss at once, simply remarking, that the symptoms and treatment are clearly laid down; and hasten to the 3d or malignant form, on which the author's remarks are both original and valuable.

A reference to the writings of Cullen, Huxham, and Heberden, will readily satisfy us that the disease which these eminent authors have called Scarlatina maligna, was by no means of a uniform type, but consisted of several varieties. Dr Armstrong conceives that the varieties included under this generic form are three in number; 1st, The highly inflammatory; 2d, The highly congestive; and, 3d, The irregular, which is distinguished by a mixture of both inflammation and congestion; being attended by venous congestions of the viscera, and by a partial or impeded arterial reaction. It is in the treatment of the two latter varieties that the greatest delicacy and discrimination are necessary, and it is more especially in them that fatal dropsies are apt to occur as the sequelæ of the disease. It is, therefore, particularly judicious in the author, to have gone at such length into the distinctive symptoms of each, and the modified treatment called for, in such cases.

Before entering farther into the subject, it is necessary that our readers should be made acquainted with Dr Armstrong's leading ideas as to fever in general, and with the stress which he lays upon want of balance between the arterial and venous systems as a cause of the early phenomena. He contends, that the first obvious operation of contagion is to change the action of the cutaneous vessels, and to cause a recoil of blood from the surface to the centre:—its second obvious effect, to diminish the sensorial power of the brain, and impair the energy of the whole nervous system. Thus both these effects tend directly to oppress the heart, and to cause a feeble action of the arteries,—trunks as well as capillaries. The consequence is, that the greater part of the blood becomes accumulated in the veins, which, from their dilatability, may be readily supposed to favour, or, at least, not to resist such an occurrence. This accumulation is sometimes so great in the larger internal veins as to overwhelm the functions of the brain, lungs, or even of the heart itself; and thus death suddenly takes place during the stage of oppression; an occurrence which has been repeatedly noticed by authors in other fevers, especially in the plague;—and which has been noticed by Dr Armstrong in scarlatina when it was epidemic a few years ago.

In general, however, the venous congestion thus arising is not so mortally overpowering; on the contrary, the blood distending the venæ cavæ, for the most part, sooner or later, rouses the suppressed irritability of the heart, and stimulates that organ to renewed exertions, which at last extend to the whole sanguiferous circle.

From this it appears, that the stage of reaction is highly salutary in the first instance, however detrimental it may be in its final effects, either by excess or irregularity.

This last conclusion the author verifies by the familiar fact, that, wherever the hot stage is not perfectly, or at least partially, developed, in febrile diseases, the danger is instant and imminent.

We have little doubt that this pathological doctrine, though it has somewhat of a hypothetical air about it, is substantially correct; at least all the phenomena countenance it; and, moreover, it is capable of direct application to practice, as we shall see when we come to notice the modification of treatment employed by the author in the congestive or irregular varieties of scarlatina.

Dr Armstrong is of opinion that fits of convulsion and shivering are means employed by nature to lessen, or remove, this venous congestion at the beginning of fevers: but the reasons which he urges in its support are very vague, and the opinion itself seems wholly conjectural. We have already stated our sentiments on this point in our review of Dr Parry's pathology; for in that work has the same opinion been advanced.

We now proceed to transcribe the author's accurate delineation of the two congestive varieties of Scarlatina maligna, beginning with the irregular.

"In the irregular congestive form the efflorescence is neither so much diffused nor of so vivid a colour, as in the simple and inflammatory modifications; and it is, besides, much more liable to disappear, and to leave a sickly pallidity of the face. Nor are the lips and edges of the tongue of so bright a red as in the two last mentioned varieties. Soon after the attack the mind often labours under dejection or alarm, which is strongly depicted on the countenance; but sometimes the mind is in a state of dulness, confusion, or indifference, and the eyes are then vacant, and the pupils dilated. Yet in some instances, the intellect remains clear for a time, though uneasiness is always felt in the head, and often a load of anxiety referred to the region of the heart. Delirium is not very common at the outset, but it often appears afterwards, and is generally a conspicuous symptom in the advanced stage. The stomach is flatulent, the belly irregular, the epigastric region frequently somewhat fuller than usual, and the stools are always unnatural in colour and smell, indicating either a morbid or deficient secretion of bile. The pulse is low and oppressed in the first stage, and it rarely acquires much firmness or fulness in the second, though it is then quick and variable. In its natural progress this form of the scarlet fever is often protracted to the end of the second week, and occasionally much longer. When it ends successfully the recovery is frequently very slow, on account of the great collapse which succeeds to the state of imperfect excitement. If it be not actively treated soon after the attack, there are, for the most part, symptoms which indicate some serious affections of the brain, liver, stomach, or of other important parts; and if these symptoms should not speedily be arrested, they may gradually or suddenly become much aggravated, and at last terminate life by coma, or low muttering delirium; by vomiting and purging; or by apparent suffocation. Towards the conclusion of such cases there are frequently appearances of a dissolved state of the blood, as inky petechiæ, oozings of black gore from the nostrils, and the like." P. 19.

With regard to the last mentioned appearances Dr Armstrong judiciously remarks, that they are nothing but the natural consequences of the preceding congestions; and that putridity or malignancy does not constitute a primary or essential part of this form, or any other form, of scarlatina. This accords with our own experience; and we believe we do not generalize too much when we say, of all fevers, that the ultimate appearances of putrescency are the product of previous augmented vascular action or congestion, aided in their operation by defective ventilation and neglect of personal cleanliness. In other words, putrescency is an accidental, rather than a necessary, result; and is often, indeed, the direct effect of warm regimen, and stimulating treatment.

"The regular congestive form, next to be examined, so oppresses the vital functions in the first stage, that there is then still greater internal remora of venous blood, and less internal and external reaction of the arteries than in the forementioned variety. The subjects of this modification are, for the most part, suddenly attacked. They become pale, faint, and sick; and chiefly complain of pain, load, or giddiness in the head; extreme oppression; and much uneasiness in the region of the heart, or at the pit of the stomach. Sometimes they at once sink as if overcome by an uncommon shock, and lie in a state of confusion and oppression, without making much complaint. At other times they walk about pale and languid, for two or three days, and then take to their beds like persons completely worn out by some great fatigue or mental anxiety. When the attack has once decidedly occurred, the respiration is either quick and anxious, or slow and impeded. There is often a mixture of livor and paleness in the face; the eyes are frequently dull, but sometimes glairy; and they acquire a fatuous or an inebriated expression in the course of the disease. Delirium, or an indifference to surrounding objects occurs; and a stupor often succeeds, under which patients finally expire." P. 23.

This form of the disease often runs its course in two, three, or four days from the occurrence of the first general oppression; the pulse is generally low, quick, and irregular, from first to last; the efflorescence is all along of a purplish or copperish hue, which deepens as the disease advances, and, towards the end, petechiæ and hæmorrhages occur.

The cold or tepid affusion, which is so highly useful in the Scarlatina simplex and Scarlatina anginosa, is quite inadmissible in the congestive varieties of that disorder. On the contrary, in the irregular congestive form, or that where the excitement is but partially developed, the author has found the warm bath and purgatives the best remedies. The first should be used twice every twenty-four hours for the first four or five days, and then only once in the same term, until there be appearances of recovery. The purgatives should be given so as to procure four or five evacuations daily, so long as the general oppression and irregular state of the excitement remain. When the pulse becomes free and full, the heat of skin equable, and the alvine secretions more natural, the cathartics should be given more sparingly; but they cannot safely be altogether omitted, until convalescence takes place.

In the regular congestive form, again,—as the danger is often imminent, prompt means must be taken to relieve the internal engorgement by the warm-bath, strongly impregnated with salt, and by soliciting the fluids to the surface by frictions with warm flannels, or warm vinegar and salt, and by giving warm gently stimulating liquids internally. Venesection, to a moderate extent, will also greatly contribute to the relief of the internal parts, by freeing them from the oppressing cause; but it should be resorted to very early, because in such cases the stage of general collapse soon succeeds to that of congestion.

Purgatives should also be freely given in this form. The author has particular confidence in large doses of calomel, either alone or combined with small quantities of jalap, so as to ensure its acting on the bowels. He declares, from much experience, that this remedy tends greatly to equalize the circulation by exciting the various secretory organs. He has been in the habit of giving six or eight grains to children, twice, thrice, or even four times a-day, and with the happiest effects.

It seldom produces ptyalism, or any other of those unpleasant effects that would result from the same quantity given in health.

We ourselves have been in the practice of exhibiting calomel largely in certain cases, and our experience confirms the position laid down by Dr Armstrong, namely, that the effect of this medicine is determined by the condition of the system under which it is given . and that, " where it has to encounter a highly febrile action, the system receives no shock from its free administration, as its main force is spent on the reduction of that excitement." In another place, he says, somewhat quaintly, that " the lancet is the right arm of medicine, and calomel the left ;" but the use of both should terminate, in every case, with the stage of excitement.

Upon the whole. the general treatment here recommended deserves our unqualified praise. Bleeding the author has employed very extensively ; but, he justly observes, that one decisive evacuation of this sort is much more efficacious, as well as safe, than several repetitions of small ones. Children do not bear great or continued losses of blood so well, in proportion, as adults, on account of the greater irritability of their nervous system.

We cannot help extracting the following remarks, which are applicable to all fevers as well as to scarlatina.

> " It is recommended by high authorities, to exhibit cathartics during the day, that, the bowels being opened before bed-time, the patient may have a respite from their operation throughout the night. Greatly as I respect some of these authorities, I cannot implicitly subscribe to this special recommendation. It is certainly highly advisable to get the bowels freely moved before bed-time; but why should the purgatives always be omitted in the night ? It is at that period that the febrile exacerbation is commonly present; and, therefore, the practitioner should not allow it to proceed unimpeded ; on the contrary, he ought to strive to lessen its intensity and duration, and this may often be most effectually done, by using both the tepid affusions and purgatives freely during the night. When this treatment is pursued, it will be found, that the patient generally falls into a quiet sleep, about the period of the morning remission ; whereas, if it be omitted, he will usually pass an uneasy or a watchful night, and be restless the greater part of the next day. It is too common a practice to administer little or no medical aid during the night, in most idiopathic fevers. But in certain cases, the loss of so much time may be dangerous, if not fatal, to the patient ; for, if the excitement should run high at that time, and not be reduced, it may give rise to topical inflammations of the viscera, which no subsequent care can remove."—P. 42.

From the specimens we have here given, our readers will be able to form a just idea of the value of this section on Scarlet fever. Its distinguishing merit consists in the clearness with which the different varieties are illustrated,—in the earnestness with which the attention of the practitioner is directed to the cold stage, (the stage so often inertly treated, although in it there is little doubt that the rudiments of future mischief are laid,)—in the absolute prohibition of wine and all other excitants during the convalescence of children, and in the strenuous condemnation, from experience, of wine, bark, and aromatic cordials, so indiscriminately recommended by almost all authors in every form of scarlatina.

The next section, on Measles, is scarcely inferior in ability to that on Scarlatina, but our limits will only permit us to advert summarily to a few leading points, referring to the work itself for the rest.

Dr Armstrong is of opinion that our treatment is, upon the whole, too uniformly active in measles, and that in the milder cases, we should be more fortunate if we interfered less with the operations of nature. We hardly see either the scope or the necessity of this general declaration; in cases of a moderate character, we believe, nature is never interfered with, unless confinement, and occasional mild laxatives, come under the denomination of such interference.

Every eruptive fever, generally speaking, has something distinctive; and it is the peculiarity of measles that the morbid actions have a tendency to concentrate themselves in the organs of respiration. This, we think, is partly owing to the eruption diffusing itself over the lining membrane of the bronchia and lungs. But in whatever manner the tendency may be accounted for, there can be no doubt that it chiefly constitutes both the present and after danger, and that it usually requires a mode of treatment, very different from that of scarlatina on the one hand, and of the pure phlegmasiæ on the other. In fact, the patient should neither be kept too warm nor too cool; neither evacuants nor cordials should be exclusively adhered to, and though laxatives are highly necessary, purgatives of the more active sort would be injurious, by preventing expectoration, and increasing the pulmonary oppression. This is an outline of the treatment that we are in the habit of pursuing, and upon the whole, that recommended by the author coincides with it in most leading points.

Dr Armstrong very judiciously puts the practitioner on his guard, against the supervention of acute or sub-acute inflammation in some internal parts, during the progress of this disease;

an occurrence which often takes place in the severer cases, and is indicated by a premature fading of the rash.

"When there are sufficient grounds for inferring the existence of inflammation in the measles, general venesection should mostly be employed as early as possible; and in those obscurer cases, where the symptoms rather indicate than reveal inflammation, the best general rule will be to try local bleeding, as this cautious procedure may do good in the beginning, and cannot do any harm. It should, however, be our constant endeavour to proportion the measure of the depletion, not only to the degree and stage of the inflammation, but to the powers and peculiarities of the patient. We shall hardly ever have occasion to regret, when we bleed at the onset of inflammatory diseases; but when we venture upon general venesection late, we most frequently risk whatever chances of recovery remain. In the commencement of inflammatory diseases, we operate on vantage ground, and may proceed boldly; but our position is reversed in the last stage, and great caution is then necessary at every step. The strength is unsubdued in the first stage, it is exhausted in the last. General bleeding arrests the topical disorder in the first stage, without weakening the system further than is necessary for the removal of that disorder; and the general debility resulting from the loss of blood, is much less than that which the unimpeded disease would have caused, not to mention the tendency of inflammation to derange the structure, or to destroy the functions of the affected viscus. But towards the close of inflammatory diseases, the energy of the heart and arteries, with that of the whole body, is wasted from the local and universal excitement, and general venesection at that time, exerts little or no influence on the topical disorder, whilst it has an inverse one on the system at large, which it immediately and mortally overpowers. Yet between the first and the last stage, and particularly in sub-acute inflammations, there is a middle one in which moderate venesection is often very useful; moreover, it is a principle of physic, that whenever any just reason exists for hesitating about the employment of general blood-letting, local should always be preferred."—P. 147.

The above extract precisely expresses our own sentiments, and we have presented it at full length, because it is more forcible than any thing that we are prepared to say on this point. It affords a lesson, too, neither unimportant nor unseasonable; for, unless we greatly mistake, there is at this moment a danger of blood-letting, particularly in general fever, being brought into disrepute, from the rash extremes to which it is carried by some of the junior members of the profession. With the adolescent medical reformer, doubt is spurned as synonymous with incapacity, and a fondness for hardy and dashing expedients, is often cherished as the indication of superior cleverness. When a spirit of professional enterprise is engrafted on a temperament naturally ardent, and when to these is added the consciousness—so flattering to the mind of youth—that he is pursuing depletory measures, from which his aged predecessors have been deterred by needless fears, or by scholastic or nosological prejudices, we need no longer wonder that the practice should be over-done. Indeed, the ultra-phlebotomist seems to forget that the blood is the organic pabulum, " vitæ nunquam non necessarium ;" nay, if we might infer his principles from his practice, he must needs believe that it is a noxious liquid, causing all manner of disorders in the solids ; and the right art of medicine consists in draining it off like dirty water ! If patients die under this work of super-venesection, 'tis their own fault ; for he, like Dr Sangrado, is ready to " write a treatise to prove they ought to have lived," and if recovery take place, it is held up as a proof of what *art* can do, although, in our opinion, it is only a proof of what *nature* can do, or rather of what she can suffer !

Surely neither Dr Armstrong nor ourselves will be suspected of being inimical to the lancet. But we consider, that we cannot better shew our high estimation of this remedy, than by guarding it from reckless abuse. Our purpose, in what we have just said, is not to offend, but to inculcate caution and discrimination ; for we know, experimentally, that few points in practice require greater nicety of discernment than to say when evacuations in fever have been pushed far enough, and when they should be laid aside.

Where inflammation, in measles, attacks the chest, Dr Armstrong has found, in addition to blood-letting, that the warm bath, strongly impregnated with salt, is a powerful subsidiary remedy When, in such cases, the patient is oppressed with tenacious phlegm, threatening suffocation, the best way of dislodging it is to exhibit an emetic. This remedy forms, also, one of the best *gargles* in scarlatina, where the affection of the throat is severe, and the accumulation of mucus distressing.

The author is of opinion that, owing to the great susceptibility of the skin to external impressions, the cold affusion must be inapplicable in measles, and he argues that it is erroneous to infer, because a remedy or process has been useful in one epidemic, that it must be useful in another epidemic of the same apparent nature. All this is doubtless very true ; but we know that the pratice he condemns was employed most extensively and successfully by Dr Magrath, in the hospital for prisoners of war at Plymouth, in the spring of 1808, and in various subsequent years, when the rubeolous contagion happened to be imported into that depôt. The patients were adults ; the treatment consisted in repeatedly washing the whole body with cold water *as often as* the heat of surface rose much above the natural standard. Even the presence of hoarseness and cough did not hinder the refrigerating process; nay, they were generally much benefited by it. No internal medicine was employed, with the exception of a saline purgative occasionally. The patients were kept very lightly covered as to bed-clothes, and restricted to low diet. The general result of this unusual treatment was, that they passed rapidly into convalescence, and that pectoral complaints, or any of the other unpleasant sequelae of the disease, supervened very seldom, indeed: in fact, the inference drawn by Dr Magrath, we believe, was, that these sequelae were arrested in their nascent state by the energetic means employed to keep in subjection the general excitement, an inference which seems highly reasonable. We content ourselves with simply stating these things as matters of fact, abstaining, for the present, from all reasonings upon them. One conclusion, however, seems unavoidable, that we have still much to learn with regard to the effects of cold external applications on internal inflammations.

The sequelae, (called in popular language *dregs* of measles,) which are so frequent and untractable in this disease, the author thinks, are eminently prevented by diarrhœa, coming on after the disappearance of the rash, or by vesicular eruptions breaking out on various parts of the body during convalescence. Therefore, when the former occurs, it should not be checked, unless it proceed to the extent of abdominal inflammation, and when the latter appear, they should never be interfered with.

> "Yet when they prove troublesome from too long a continuance, or where they proceed from some disorder of the digestive organs, the warm bath, and the moderate administration of the Harrowgate sulphureous water, will remove them with the greatest safety; and I have found these two expedients also of the most advantage in those chronic glandular complaints which sometimes follow this distemper, and which are sometimes connected with a vitiated state of the abdominal secretions." p. 176.

The next section, consisting of more than 150 pages, treats of chronic inflammation of the bronchia, ulceration of the trachea, chronic inflammation of the pleuræ, and tubercular consumption. These subjects are handled in a manner that stamps Dr Armstrong as a very accurate observer. He has laid down, with great minuteness, the symptoms that distinguish these affections from each other; but we have great doubts of their being constant, or of its being possible, in the majority of instances, to establish an accurate diagnosis.

On the subject of phthisis, there is a great deal of valuable remark. Tubercles he considers to be formed by exudations from the minute vessels in the cellular tissue of the lungs:

these exudations, though becoming partially organized, may be considered almost extraneous substances, from their interrupting the pulmonary circulation, and acting as local irritants. Even when in themselves disposed to quiescence, they are frequently roused into activity by some independent or distant irritation. In this chapter, it is the author's leading object to direct the attention of the faculty to the state of the skin, which, he thinks, has been too much neglected both in the pathology and treatment of phthisis. The following paragraph is every way deserving of attention:

> "Certain conditions of the skin are more frequently connected with the rise and progress of phthisis than perhaps any of the above noticed irritations, and it is to this organ unquestionably that we must often look for the commencement of those morbid movements which ultimately undermine the fabric of the lungs. We cannot put a needle into any point of the skin without drawing blood, and exciting pain, which demonstrates it to be an extremely vascular and nervous tissue. In fact, we may consider it as an expansion of larger or minuter vessels and nerves; so completely are they interwoven with every fibre of its substance. From such an union of two distinguished textures, one might naturally suppose the skin a most important organ, as physiology has shewn that, to those parts the most highly and perfectly organized, the most important offices of the economy are committed. The whole extent of the skin is perpetually exposed to the action of surrounding agents; and between it and the central parts, an intimate sympathy exists, but especially between it and the lungs; for not only do the skin and the lungs mutually compensate a deficiency or an excess in their respective exhalations, but they are likewise closely connected by a free interchange of vessels, so that when a reduced temperature diminishes the action of the skin, it at once increases that of the lungs; and the contraction of the vessels on the former in some degree congest those of the latter. Who are the persons most liable to tubercular phthisis? Those who have delicate skins, and who are exposed, without sufficient clothing, to the vicissitudes of the weather. Nay, if we go more minutely into the subject, we shall find that many diseases of the skin are incompatible with those of the lungs; that is to say, certain excitements of the first organ often prevent dangerous affections of the last. Hence it is, even in Great Britain, that those persons afflicted with cutaneous diseases are the least obnoxious to pulmonary consumption; but let their cutaneous diseases be incautiously cured, and they often afterwards fall victims to suppuration in the lungs, as I well know from personal observation." p. 211.

As a natural inference from the foregoing considerations, Dr Armstrong lays particular stress, in the treatment or prevention of phthisis, on such means as act powerfully on the cuticular function, such as counter-irritants, frictions, emetics, a regulated temperature, and flannel clothing, or fleecy hosiery worn constantly next the skin.

Many interesting observations follow, relating to medical police, and to the habits and diet of the lower orders in our manufacturing towns, as predisposing to pulmonary consumption. In this part of the work, also, the author takes up the hopeless subject of female dress; which, from its unsuitableness to our changeable climate, he forcibly points out as a most frequent cause of phthisis, particularly in strumous habits. This is an old grievance; it has already bid defiance to the remonstrances of both moralists and physicians; and it would, we apprehend, set at nought even the injunctions of a sumptuary edict. We have no wish to throw away our own admonitions on this matter, because we are well convinced that the power of the reigning folly is too great to be put down by the denunciations of a set of reviewers, the very nature of whose office places them literally in that obscure class, "*whom*" (according to the phraseology of fashion) "*nobody knows;*" and because, were we bluntly to speak our sentiments, we are fearful we should be guilty of lese-majesté towards "*l'adorable moitie du genre humain.*" Nevertheless, (since example teaches more strongly than precept,) we shall quote an interesting fact recorded by Dr Armstrong, which must have weight, if any thing can have weight, with the fair votaries of fashion.

> "Though having practised much among the Society of Friends, it is remarkable, that while at Sunderland, I only attended one young woman of their denomination who died of pure consumption; and as this disease was not uncommon amongst other young women of that place, the comparatively greater exemption of those of the Friends appeared partly to depend upon the more prudent mode in which they clothed themselves." p. 332.

We have next a few pages on the author's experience of the utility of balsam of Copaiva in the inflammations of mucous membranes. He was first induced to make trial of this medicine in the early stage of gonorrhœa, by the advice of Dr Pearson Dawson, who had been long in the habit of so employing it. The result of this trial in Dr Armstrong's hands was so favourable, that, ever since, it has not only been his favourite remedy in gonorrhœa and leucorrhœa, but has been extended with very great benefit to chronic inflammation of the bronchia and trachea, and even to phthisis pulmonalis. Since this balsam produces such striking amendment in the inflammatory affections of some mucous membranes, he is naturally led to inquire whether it may not be productive of the same beneficial effects in all of them. This question he has had no opportunity of deciding experimentally; yet he strongly recommends the medicine to be tried in croup and dysentery, (conjointly with bleeding and purging,) and also in spasmodic asthma, and hooping-cough. Whether it may be productive of all the good consequences in the latter diseases that the author anticipates, or not, there can be no doubt, that, even in gonorrhœa only, it must be a very gratifying improvement. When we recollect how vehement the symptoms often are, and how long the discharge is sometimes protracted, particularly in scrofulous habits, a drug which is declared from experience to possess more power over this complaint than any article in the pharmacopœia, and which cures the milder cases in two or three days, and the most violent ones within a fortnight, must be accounted no inconsiderable boon to the practice of physic. It will also effectually supersede the use of astringent injections, which are so often productive of painful stranguries, obstinate gleets, or troublesome strictures of the urethra. We shall subjoin the formula of preparing the copaiva.

> " As its powers may be much impaired by improper combinations, it should always be blended, in soft distilled water, with mucilage of gum arabic, or with the yolk of a newly laid egg. The following formula answers very well in gonorrhœa : Take about a hundred, or a hundred and twenty drops of copaiva, rather less than two ounces of mucilage, two ounces of soft distilled water, and two drachms of refined sugar. The copaiva and the mucilage must first be most intimately rubbed together in a marble mortar ; the sugar must afterwards be dissolved separately in the water ; and then these two mixtures must be well incorporated, by small portions at a time, until an entirely smooth and uniform emulsion be made. One half of this emulsion is to be taken before breakfast, and the other half immediately before supper. It is most frequently advisable to eat a small aromatic spice-nut before and after each dose, and one or two during the day, more effectually to guard against disturbance of the stomach. On some occasions the doses of copaiva will have to be increased to three, four, or five drachms in the twenty-four hours, until it answers its specific effect." p. 352.

The medicine is advised to be persevered in for about a week or ten days after the disappearance of the symptoms, otherwise a relapse may be anticipated.

The last section of the work, consisting of nearly 100 pages, treats of " Chronic Diseases and Sulphureous Waters ;" and we think Dr A. is entitled to the praise of having shed a good deal of new light on the pathology and treatment of this obscure and defective branch of medicine.

The treatment of acute diseases has been manifestly much improved of late years ; and even in chronic ones, the advancement, though less conspicuous, is by no means inconsiderable.

The doctrine by which they were wont to be ascribed to depravation of the solids, or to laxity of the moving fibre, seems to be gradually giving way, and better views are beginning to obtain. We now-a-days seldom see a physician prescribing solely for debility in chronic affections: there seems a tendency, little known formerly, to regard it as a symptom only, and to prescribe for the states on which it depends, and which must be removed as a necessary preliminary to recovery. In our own practice, we have often removed a congeries of most distressing anomalous symptoms, (among which debility was the most prominent,) solely by brisk purgatives, steadily pursued every other day, for a few weeks; supporting the general strength, at the same time, by an allowance of dry, nutritious, but simple food. In fact, we are firmly persuaded, that many lingering disorders of apparent debility arise from a state of vascular fulness in some vital organ, particularly the brain and the heart; so that the efficacy of purgatives, (often attempted to be explained by the theoretical axiom of their " setting up a new action in the system,") in reality consists in their depletory effects,—in their removing from certain important organs the excessive onus of circulating fluid,—thus withdrawing irritation, and equalizing the distribution of the blood. Our limits will not permit us to pursue these observations; we therefore pass at once to Dr Armstrong's pathology.

> " Between chronic and acute diseases a much greater affinity perhaps exists than has usually been conjectured. The first stages of both consist in disordered action, the last generally in deranged structure. If high degrees of venous or of arterial fulness mark acute diseases, low degrees of the same condition mark chronic diseases; and these degrees may mutually pass into each other, so that an acute disease shall become chronic, or a chronic disease shall assume an acute character.—With confidence, then, it may be affirmed, that chronic complaints are almost always connected with either venous fulness, or with arterial excitement. Wherever venous congestion exists as the cause of chronic disease, it mostly, in the end, occasions an increased action of the heart and arteries; but this increased action is frequently imperfect and irregular for a considerable period, so that there is fever at one time, and collapse at another, and between these two states the health is suspended. In fact, whenever the increased action of the heart and arteries is permanently established, then the venous congestion may be said to terminate in a new disease; since an increased action of the heart and arteries is one of the natural cures of venous congestion, and an augmentation of the secretions is the other." p. *365*.

The author thinks there is an identity or consent of action between the lungs, skin, kidneys and intestines, so that increased exhalations or increased secretions from any one of these organs often remove congestion, and thus compensate for arterial excitement. In this manner he accounts for those diseases that pass under the name of hypochondriacal, hysterical, nervous, and so forth; and, indeed, for almost all chronic complaints, save such as arise from lesions of structure.

We readily concede to his views the praise of extensiveness, and, in many instances, of correctness; but we can see no reason, except a fondness for consistency in speculation, why he should always confine primary congestion to the veins. We think facts bear us out in maintaining, that in some chronic disorders there is as great a torpor, and consequent congestion, originally observable in the capillary arteries, as in the veins. We cannot, therefore, agree with the author that the arterial congestion is always the product of excitement of the heart. We also consider his system too narrow, inasmuch as it attributes too much to particular states of the blood-vessels, and too little to those conditions of the nervous system which are now and then the primary cause of various obscure affections, both of the head and of the alimentary canal. Much of what we urged in our analysis of Dr Parry's Work against simplicity as the ground-work of pathological systems, is applicable here; to that analysis, then, we beg to refer.

Let us now follow Dr Armstrong to the conclusion. He divides all chronic complaints into two classes; 1st, those of chronic congestion; and, 2d, those of chronic excitement of the heart and arteries; and goes on, with his usual ability, to illustrate that this second series may not only arise indirectly from venous congestion, but directly from over-stimulation, by the excessive use of ardent spirits, and by the excess of nutrition consequent upon luxurious habits. The chronic excitement, or slow fever thus induced, ends ere long in chronic inflammation; and ultimately, unless checked, in organic derangement. He also points out how wine and opium, so often prescribed for debility during this state of things, tend to augment the excitement, and to fix it in inflammation. Calomel also, though so highly beneficial in acute diseases, is very often hurtful when freely used in chronic ones. Improper treatment during convalescence from acute attacks, is another fertile source of chronic ones, as it produces the direct excitement so often spoken of. We shall extract the following paragraph at full length, as it is an admirable one.

> " When patients are convalescent from acute diseases, it is an established rule among many practitioners, to allow them a generous diet; and some go even farther than this, as they prescribe wine and what are called tonics, with the hope of more rapidly recruiting the

strength. It is not intended in these pages to insinuate any thing against others, for this was the practice which I once pursued myself; and it was from observing its bad effects, in many instances, that I was led generally to abandon it, and to substitute one more consonant and salutary to the system. When any acute disease has subsided, no matter where situated, it commonly leaves a tenderness or weakness in the organ which had sustained the main attack; and it is highly probable that the capillaries of that organ had undergone a change during the disease, and that some of them may have been closed or obstructed in such a manner as to modify the future circulation of the part. If the circulation of any part be already less healthy than the rest, increasing the action of the heart must propel more blood in that direction; and the blood thus propelled may reproduce disease. This is not theoretical speculation, for I have known many cases of chronic inflammation arise after acute ones, from following the stimulating plan during the appearances of convalescence; and since I have adopted a more cautious, and in fact a different system, such occurrences have been comparatively rare in my practice. After all great disease, the constitution falls into a state of collapse, from which it is gradually recruited by the innate powers, provided proper support be given; but if, on the one hand, we give too spare a diet, great bodily and mental irritation succeeds; and if, on the other, we give too full a diet, the heart and arterial circle are stimulated to excess, and an acute, sub-acute, or chronic inflammation, may supervene. It is never safe to attempt to rouse the constitution at once from collapse into strength. In reality it cannot be done; for when the powers are over-exhausted, their vigour can only be gradually restored. From long and unbiassed observation, I am fully convinced, that most of the medicines called tonics are either useless or pernicious; and if these were erased from the pharmacopœia, it would be a real benefit to the profession and to mankind, for they only serve to mislead the former, and to tantalize or injure the latter. Tonic medicines generally oppress the digestive functions, or operate as direct stimulants; and in either case they are improper. So far from such drugs being appropriate to a stage of convalescence from acute disease, mild laxatives are most frequently requisite to preserve a right balance between the ingesta and the egesta; and the practitioner who substitutes the former will find that his patients will pass better through convalescence, and be afterwards far less subject to consecutive attacks of inflammation." p. 382, *et seq.*

The summary of Dr Armstrong's doctrine, then, is, that we must cease to regard chronic complaints as diseases of real debility; and that we must treat them by depletion, with a view of relieving that vascular congestion in the viscera on which they depend. By timely evacuations the heart regains its power " like a spring, when the superincumbent pressure is withdrawn." Yet venesection must not be practised unless early, cautiously, and sparingly: if the pulse rise after it, we may be sure it is answering a good purpose; if, on the contrary, it become weaker, it is a sure indication that this plan should not be pursued farther. Purgatives, blisters, and alteratives, form the remedies most generally applicable; but as to the last, mild and short courses of mercury answer much better than those of a contrary description.

The author has ascertained, by extensive observation, that the sulphureous springs of Harrowgate and Dinsdale possess highly beneficial powers in arresting chronic inflammation. This effect is not owing to their purgative action on the bowels, but to the sulphuretted hydrogen which they contain. Dr A. conceives the latter agent to be superior to mercury, inasmuch as it possesses equal power in exciting the secretions; while it does not, like mercury, exhaust the system; but, on the contrary, braces and renovates it. The relief which it affords in all chronic cases of congestion or of excitement, and in chronic inflammation of the liver, in phthisis and other untractable disorders, has been very conspicuous; while in the various, and frequently obstinate, diseases of the skin, it has far more efficacy than all the complicated formulæ of art. Upon the whole, from our own observation, we entertain a high opinion of sulphureous waters; and, although our experience of their effects in many of the complaints mentioned by the author is very incomplete, we, nevertheless, think them well deserving of farther trials. We therefore cordially second his recommendation that they should be prepared artificially, and introduced into ordinary prescription, in lieu of some of those medicines which have been so long employed to no purpose, in the class of diseases alluded to.

Though our analysis has run to so considerable an extent, we can assure our readers that we have given a most inadequate view of this valuable volume. Every where crowds of important observations have been unavoidably passed over. It is an excellent and a practical work; and, on consideration, perhaps we ought to retract, or qualify the assertion we set out with, that it is unequal to his former one. In truth, we suspect the inequality lies rather in the style than the matter; for the language of the present volume is loose, and often careless. Repetitions, too, of nearly the same sentiments, occur much oftener than they should do. Our opinions in regard to this work were derived from a diligent perusal of the first edition, and, although a second has appeared before they could be laid before our readers, we do not find any considerable improvement, nor could we expect it. We trust, however, that this almost unprecedented success will stimulate our author carefully to revise it; and we are confident that he could condense the whole matter into one-third less bulk.

II.

On the Nature and Treatment of Tetanus and Hydrophobia, with some Observations on a natural Classification of Diseases in General. By Robert Reid, M. D. Licentiate of the King's and Queen's College of Physicians in Dublin, Member of the Royal Medical Society of Edinburgh, &c. Dublin, 1817. pp. 136.

This little treatise, of which a part first appeared in another form in the Transactions of the College of Physicians of Ireland, Vol. I., is divided into three chapters, and a short appendix. The subject last enumerated in the title-page occupies the first part of the book, and presents us with the author's ideas on the classification of diseases, according to the nervous masses by which the organs are supplied, viz. the brain, spinal marrow, and ganglions. The principles of this classification are subsequently applied to the pathology of tetanus and hydrophobia. As we do not intend to enter into any theoretical discussions at present, we shall confine our critical analysis principally to Dr Reid's practical remarks; and of these we shall chiefly select such as illustrate the morbid anatomy of tetanus; for, from no inconsiderable experience, we are assured that it is in the dissecting room principally we can arrive at any just pathological views on the subject. Speculations are endless; the disease affords food for them even to satiety; and to how little purpose they have hitherto been pursued, must be quite apparent to any cool and reflecting man who opens any one of the countless treatises which have been written upon nervous diseases, from the time of Hippocrates to the present day.

The spine is the point to which Dr R.'s views are directed. In this he is by no means singular, for we know it to have been the object of particular inquiry among several well informed practitioners for some years past; and we must confess that it has long appeared to us to be the situation in which the morbid anatomist is most likely to be repaid for investigations which, from the very nature of the dissection, must be more laborious than those in any other part of the body. We claim no originality in this; and we are by no means inclined to concede it either to Dr Reid or to any other British physician; although the Doctor seems to think that the state of the spine has not been taken notice of by any modern except himself. Now, not to mention Burserius, from whose Institutes a long passage is quoted by Dr Reid, in which reference is made to Fernelius, Morgagni, Lieutaud, and Valsalva, besides others less known in this country, surely few well informed physicians, though they may not have had access to the admirable dissertation by Dr Frank, ("De Vertebralis Columnæ in Morbis Dignitate," Pavia, 1791,) can be ignorant of its existence. Since that work was published, many teachers and writers in Italy, France, and Germany, as well as in our own islands, have turned their attention to the subject.* Periodical essayists and debating societies have also of late been much occupied with it; and without any precise theory having been publicly promulgated from the press, every one knows, that for some years, it has been quite a medical rage to refer all diseases to the spinal marrow, whatever may have been their nature, symptoms, or degree; and the junior proselytes to this doctrine are as active and clamorous in the enforcement of their opinions, as was the "Toinette" of the inimitable Moliere, in representing the lungs as the seat of the countless maladies concentrated in the person of Monsieur Argan;

"Le poulmon! Le poulmon! Le poulmon! vous-dis-je."

The following is Dr Reid's account of the morbid appearances.

"In the first two patients, which I had an opportunity of examining, the disease did not prove fatal, until the fourth day. No morbid appearance could be discovered in the viscera of the thorax or abdomen. The only appearance of disease that could be observed in the brain, was some increased vascularity, particularly in the membranes; but there was a considerable quantity of water flowed from the spinal canal, after the brain had been removed.

"On the 25th of February, having occasion to visit, about three miles from town, I was requested to see a boy, aged thirteen, who, the night before, had received a severe burn in the toes of the left foot. I was informed he was a beggar, who went about the country, exposed to the vicissitudes of the weather, during an inclement season; and that the evening before, he went to rest in a lime-kiln, where he was found, the next morning, in the state I saw him. Having no friends, nor habitation, I directed that he should be sent to the Richmond Surgical Hospital, where he remained under treatment for four or five days, when tetanus came on, with the usual symptoms, and in thirty-six hours after the first attack, he expired. I did not, however, see him again until after he died.

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* We particularly allude to the Lectures of Dr Saunders, delivered in this city, we believe since 1808. And we would refer to the valuable paper of Dr Abercrombie, in our last volume, for notices of many other authors besides those already named.

" On examination of the body, after death, the viscera of the abdomen and thorax appeared perfectly natural, and there could not be discovered any morbid appearance in the muscular parts. The brain appeared healthy in every respect, except some increased vascularity in its investing membranes.

" Upon opening, however, the cavity of the spine, from the back part, there appeared to be a fatty substance, deposited in the cellular tissue, investing the dura mater, for the extent of several of the dorsal vertebræ. On raising the nervous mass (with its dura mater entire) from the spine, there appeared a considerable effusion of blood into the cellular tissue, connecting it to the upper lumbar and lower dorsal vertebræ. A similar effusion occurred also along the bodies of the upper dorsal, and two inferior cervical vertebræ. On slitting up the dura mater, on its anterior surface, or that next the bodies of the vertebræ, the nervous mass appeared highly vascular, and the vessels, of every description, remarkably tortuous. In the part which was situated opposite to the ninth and tenth dorsal vertebræ, there appeared a whitish substance, very nearly resembling the medullary matter effused between the arachnoid coat and pia mater, occupying the space of about an inch and half, and covering about half the circumference of the nervous mass. On breaking the membrane enclosing it, I could wipe it off, and there could not be the slightest rupture discovered in the pia mater, or any of its vessels. The only appearance in the nervous substance itself, that I could discover, was a deeper tinge than natural, in its cortical and medullary parts." p. 34—37.

Dr Reid illustrates this last dissection with a plate, which certainly we cannot compliment as any proof of graphic skill. He then proceeds to draw some practical conclusions from his dissections, and those which he quotes from Burserius. The sum of them is,—that the disease is of an inflammatory nature, and that its principal seat is in the membranes investing the nervous mass of the spine, the vessels of which present the tortuous appearances characteristic of membranous inflammation; the peculiar characters of the opisthotonos, emprosthotonos, &c. &c. will, he thinks, be determined by the situation in the spinal canal, which is occupied by the effusion. Watery, or other effusions, will be thrown out into the canal, when the disease has not been so violent as to destroy the patient before the third day, and may always be expected to occur where it has been of long continuance; and lastly, he mentions, that irritating the spinal marrow of a decapitated animal, without compressing it completely, throws the muscles into a state resembling tetanus.

We find but one observation which strikes us as new and remarkable, in his section on the "Causes of Tetanus." It is this: That soldiers are more liable to tetanus than sailors; to which it appears to Dr Reid they may be very much predisposed, in consequence of their wearing knapsacks! Horses also, he assures us, are more subject to this disease than other animals, from analogous reasons. As we conceive this to be a very curious speculation, we shall extract the passage, in hopes that some active measures may be taken to unload the spine and its vessels, by those official persons to whose province it peculiarly belongs to attend to those weighty concerns; contenting ourselves with suggesting the propriety of transferring the burdens to the bellies of the men and horses; this might diminish the chances of tetanus, and it might moreover tend considerably to obviate the calls of hunger by the pressure on the stomach, after the fashion of the Indian warriors.

> "The exposure of the body to the vicissitudes of the weather, appears to be the most general predisposing cause to this disease. Thus we observe, that in the army it is prevalent during the campaigns, where the men have been exposed to the inclemencies of the season. It is, however, a remarkable circumstance, that this dreadful malady is very seldom met with, even in its slightest stages, among the navy, while it is found generally fatal to military men." p. 46.

The Doctor accounts for this by the regularity of the naval service, which habituates the seamen to the vicissitudes of the weather, while soldiers, though defended in barracks, are much exposed in an active campaign. He then proceeds to the affair of the knapsacks.

> "It may be useful to take notice here of another circumstance which gives a predisposition in soldiers to this disease, and to which sailors are not exposed. We observe that soldiers are unavoidably obliged to carry a certain portion of their luggage in their knapsacks on their backs. Now we know that, in an active campaign, it is impossible for nature to adapt itself to the various changes which must occur in the temperature of the parts in the vicinity of the spine, by wearing the knapsack or not, at such uncertain periods.
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> "Indeed, circumstances analogous to this would seem to account for horses being more liable to this disease than any other quadruped."

We do not find that military officers are less liable to tetanus than their men, although they carry no loads on their backs; but we believe we have found a clue to this ideal naval exemption. Dr Dixon, whose admirable paper in the Medico-Chirurgical Transactions is known, no doubt, to almost every British surgeon, has observed, that tetanus was much less common in the fleet in the West India islands of *late years* than formerly, when Sir Gilbert Blane, and other able physicians, wrote their accounts of the diseases of seamen. This he attributes, not to any difference in the physical constitutions of seamen, or in their modes of living, &c. &c. but to improvements in the treatment of their wounds, medical as well as operative, and particularly to the greater attention paid to the state of their bowels. This, we believe, to be the foundation for the idea of sailors being less liable to tetanus, after their wounds, than soldiers; at least, if any other grounds exist, we must profess ourselves ignorant of them. Although Dr Reid takes very little notice of the distinction between idiopathic and symptomatic tetanus, a matter which we conceive to be of the utmost importance in the treatment, yet, in common with all other writers, he observes, that it most generally happens in consequence of wounds. We confess, that had we not his assurance that this dreadful malady was seldom met with among seamen, *even in its slightest stages,* we should have been led to look for it particularly in them, considering their activity during war, and recollecting what we have seen in the journals of many naval surgeons. On referring to some of these which have been placed within our reach, and particularly on again consulting Dr Dixon's communication, we find, that this supposed exemption did not exclusively apply to the navy, but *was extended also to the army* in the West Indies, as may be seen in the letter of Dr M'Arthur annexed to Dr Dixon's report. *

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* Med. Chir. Transactions, Vol. VII. p. 476. Sir Gilbert Blane informs us, that the number of wounded in the actions of April 1782, in the West Indies, was 810, of whom 20, or one in 40.5 were attacked with tetanus, 17 of whom died, (*vide* Diseases of Seamen, p. 3. chap. 4.) In the army in the Peninsula, from December 1811 to June 1814, 20.886 wounded soldiers were admitted into the regimental hospitals. Sir James M'Grigor has not been able to state to us particularly how many of these died of tetanus; and, indeed, considering the multiplicity of hospitals, their dispersion over an extensive country, &c. &c. it is little to be wondered at that he could not. But we find in his valuable Tables, (Medico-Chirurgical Transactions, Vol. VI.) that no more than 506 of these men died of their wounds. Now, if we suppose that *every one of these individuals* was carried off by tetanus, still it will not amount to the proportion observed in the navy, being little more than 1 in 41.

Making every allowance, however, for the West India climate, &c., on one side, and the deaths that may possibly have happened in general and detached hospitals, or which may have been unnoticed, on the other, still we have no reason to suppose that soldiers are more peculiarly liable to the disease than sailors, or that it is slighter, or less fatal in one service than the other. We may here remark, that it is well known to practical men, that tetanus occurs much oftener in some seasons than in others, and in certain districts, or even hospitals of the same town, than in others, (*vide* Hennen's Military Surgery.)

We now turn to the section on the treatment of tetanus. Now and then we hear of cures, but they are "few and far between;" and, we believe, that those military practitioners who are the best acquainted with the complaint, and whose experience is recorded in the valuable paper of Sir James M'Grigor on the Diseases of the Army in Spain, are those also who have the least sanguine hopes of its admitting of a fortunate termination.* Of the comparatively few successful cases on record, perhaps not one-half have been treated upon the principles deduced from any preconceived pathological views, but the "methodus medendi" has been taken up on the purely empirical ground of experience or imitation. We would by no means wish to damp the spirit of investigation by this remark, or by any other which may fall from us in this paper; our object is rather to excite it,—to direct it to the proper channel, and to keep it steadily in that direction, undisturbed by the effervescences of fancy, and unbeguiled by that destructive mental "Will o' the-wisp," premature generalization.

In justice to Dr Reid, we shall give an outline of his theory of tetanus before proceeding to his plan of cure. Some muscles, he observes, resist the tetanic spasms for a long time, viz. those of the thoracic and abdominal viscera, and those which belong to the five senses; thus, though vomiting sometimes occurs it seldom continues; the appetite remains;—digestion and secretion are carried on;—and, during the remissions of spasm, circulation and respiration usually return to their natural state. All this shows that the ganglionic system is little affected. The tongue retains its mobility for a considerable time; the arms are not affected till long after the lower extremities; and then those muscles which move the fingers are the longest exempted from spasm. Besides this, there is seldom delirium or even confusion of thought. This shows that the cerebral system and organs of the senses are little affected. Again, he remarks that the only parts of the body affected with the disease are the muscular; all the nerves distributed to these parts, and which are the proper stimulants to muscular action in the living body, take their origin from the nervous system of the spine. No derangement is

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Yet, even admitting that the year 1782 was peculiarly productive of tetanus, and the years 1812, 13, and 14 peculiarly the reverse, still we shall find that there are no grounds for saying, "that it is very seldom met with, even in its slightest stages, in the navy." Indeed, we know, that after the action of Copenhagen in 1801, as large a proportion of tetanic cases occurred in the fleet, as after any land-battle during the war.

* Medico-Chir. Transactions, Vol. VI.

found on dissection in the structure of the muscles, hence it must be in the nervous system of the spine. He conceives, that the other systems, so far from being the seat of the disease, have rather a tendency to oppose it; but what seems, in the Doctor's mind, to set the theory beyond dispute, is the examination of the body of the wretched beggar boy, already detailed.

Dr Reid himself observes of this dissection, "that the remarkably circumscribed appearances of both the extravasations of blood, would at first lead one to imagine that they were the result of local injury." To this opinion we do "most seriously incline," considering that the unfortunate pauper was without friends or habitation,—exposed to every species of accident, and at length reduced to the dire necessity of going to rest (as the Doctor expresses it) in a burning lime-kiln. It is much to be lamented that the state of the nerves from the injured extremity up to their spinal origins was not examined; this would have given probability at least, if not a perfect confirmation, to Dr Reid's opinion.

We are by no means inclined to withhold from Dr Reid an allowance of praise for his chapter on the morbid appearances; for the dissections which he gives, and all other dissections in which we do not mistake the *effects* of disease for its *cause*, are of great importance to assist in yielding materials for the foundation of a rational theory, and of an effectual mode of cure. But it is not upon one solitary dissection, however scientifically conducted, nor upon the history of one or one hundred cases, however striking in their details, that we are to commence a premature pathological superstructure. How admirably has the illustrious Stewart pointed this out to our observation, and how happily has his vigorous language been adopted in the preface to the Transactions of that very association to which Dr Reid's paper on tetanus was originally read. The enlightened practitioner, it is remarked, "distrusting conclusions which rest upon this or that individual case, is anxious, by combining those of an immense multitude, to separate accidental conjunctions from established connections, and to ascertain those laws of the human frame which rest on the universal experience of mankind." * On examining the annals of physic, we do not find that the most enlightened practitioners have been the most distinguished for the boldness of their theories, or for their readiness in forming them; nor, indeed, is every medical philosopher, even among the ingenious, qualified for minute inquiries in morbid anatomy, and particularly for spinal dissections. The author himself, in his note at page 36, furnishes us with a striking lesson of the necessity of a pathological anatomist being intimately acquainted with the sound structure before he undertakes to theorize upon the morbid. "Since writing the above,' says the Doctor, alluding to the case of the boy formerly quoted, "I find that this fatty substance is *natural to the part.* It was taken notice of in the examination of this case on account of its being absent at both parts of the column exactly opposite to where the extravasation was afterwards found, on raising the mass from the vertebræ." On this we shall observe no farther. It speaks for itself, and it assuredly harmonizes very badly with a subsequent attempt to correct the common descriptions of the accessory nerve of Willis, given at page 94. This, the Doctor in another note informs us, he has not yet found accurately described in any English author.

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* Stewart's Philosophy of the Human Mind, Vol. II. p. 469, 2d Edition, as quoted in the preface to the First Volume of the Dublin Transactions.

Dr Reid asserts positively, that the thoracic and abdominal viscera are never primarily affected, and that the disease *cannot take its rise in the nervous substance supplying these organs.* (p. 26.) Some authors, indeed, he observes, have mentioned morbid appearances in these parts, but they were evidently the effects of the disease, or of the remedies employed for its relief. Now, it so happens, that there are cases on record in which primary affections of the par vagum, and of the phrenic nerve, have been the causes of tetanus. We have not an immediate opportunity of reference to the works of either Meyer or Vetter to give the cases or observations on them at length; but the former author states an instance of that disease proceeding from an ossification of the pleura irritating the phrenic nerve; and Vetter gives another instance in which an irritation of the par vagum from an ossified gland in the trachæa produced the same effect.* We have ourselves seen, or had access to the knowledge of various instances of tetanus, produced from injuries of the jaws and neck. It may be said that, in the latter situation, there is room for doubt as to the particular nerves which may have suffered the injury; the phrenic, or the recurrent, or the origins of the axillary plexus, &c &c. may have been affected; but tetanus has occurred under less equivocal injuries or irritations of the nervous substance, supplying the organs in the thorax and abdomen. Baron Larrey and others give instances where injuries of the pharynx by foreign bodies sticking in it have produced the disease. Intestinal irritation is a common source of it in children; and, perhaps, the most common of all, in that species of it, the lock-jaw of new-born infants. With regard to the injuries of the cerebral system, including the intellectual powers and the five senses—to what, we would ask, are we to refer the instances of tetanus from terror and from sympathy, on seeing others under the disease, if not to a certain degree of affection of the intellectual powers? We would by no means be understood as denying that, in a vast majority of cases, tetanus proceeds from injuries of the extremities, and principally affects the muscular parts; but it has also proceeded from injuries of other parts, rare though they have comparatively been. It has proceeded, as Dr Reid himself states, (p. 51,) from a slight stroke of the lash of a whip under the eye, which did not even break the skin. The extraction or insertion of a tooth has produced it in some instances; and the cases are extremely numerous, in which the injury has been so slight as not at first to have been felt beyond the sentient extremities of the nerves which form the organs of touch in the fingers. How, then, can that doctrine be maintained which denies the *possibility* of its production from affections of the organs of sense, or of any other system of nerves, but those proceeding from the spine? We have already professed ourselves convinced of the importance of inquiries into the state of the spine in this disease, but to make it *exclusively* answerable for all cases, appears to us to be a position altogether untenable in the present state of our knowledge, and likely to call down upon it the most untoward visitation that can possibly befal a theory, the indiscriminating ridicule of the thoughtless, and the silent pity of the thinking part of the profession.

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* Sammlung med. Beobachtungen, p. 676; and Vetter, Aphorismen, p. 131, referred to by Ploucquet.

We shall pass over the diagnosis and the prognosis, to come to the treatment; and here Dr Reid presents us with nothing novel in the mode of cure, or at all corresponding with the expectations we had formed from his previous pathological views of the cerebral, spinal, and ganglionic systems. He furnishes us, however, with a recapitulation, which we shall transcribe:—It is prefaced by a remark, the veracity of which we are by no means disposed to question, viz. "that many particular circumstances may occur, which therefore ought to be left to the judgment of the practitioner."

> "When tetanus is idiopathic, our first proceeding in the treatment should be to apply a blister along the whole length of the spine: for we find, that, in this case, the disease generally runs through its course with the utmost expedition. When, however, it has been induced, by an extensive injury, and that, from what has been stated, it would appear disposed to take on the chronic form; there is every probability advantage would be derived, by putting the patient under a mercurial course, and the blister may be held in

reserve, lest the disease may suddenly gain strength, before the mercury could take effect.

"The bowels should be evacuated once a day, or at least every second day, and when necessary to employ medicines for this purpose, calomel should be a principal ingredient.

"Copious general perspiration, which is most powerful in reducing the force of the disease, should be excited by the compound powder of ipecacuanha, (occasionally assisted, when necessary, by James's Powder,) and may be given in quantity, of from five to ten grains, every third or fourth hour.

"When the cerebral functions are threatened in this disease, we must support this system by stimulants. The Madeira wine, or something of that nature, is the best, and should be given in small quantities at a time, which may be frequently repeated, not, however, so as to cause intoxication." p. 76, 77.

We have a very few remarks to make on the theory and the treatment.—It has been our lot to see, or obtain correct notices of, perhaps, one hundred cases of traumatic tetanus; in no two did we ever find the symptoms exactly alike; and they most obstinately and pertinaciously resisted all attempts at classifying them under distinct or separate heads, or affections of systems. We have seen all the systems equally and synchronously affected. We have constantly seen the diaphragm among the first parts attacked, and pain from the scrobiculus cordis, along its whole course, the most urgent symptom. We have repeatedly remarked affections of the œsophagus, and of the muscles of deglutition, as the leading morbid phenomena. We have also seen severe retching, and even vomiting, from the very commencement to the end, in several cases; and, in almost every instance, we have remarked violent spasmodic affections of the intestinal canal, together with various other convincing and early marks of ganglionic and cerebral affection, and all these seeming to precede, rather than to be influenced by, the spasms of the muscular parts. We assuredly have also seen a progress of the disease the reverse of this. Dr Philip's experiments certainly show us, that on slightly touching with a wire the cut end of *the spinal marrow* of a decapitated animal, after the violent spasms had ceased, the muscles of voluntary motion are immediately excited into action; but they also show us, that, on approaching that part *of the brain* whence the spinal marrow and nerves originate, violent spasms are produced.* We are not aware what effects would ensue on applying stimuli to the fore or back part of the spinal marrow, whether opisthotonos or emprosthotonos; but we are assured by the eminent physiologist above quoted, that the convulsions produced by opium, which cannot be supposed to act upon one part of the spine more than on another, invariably assume the form of opisthotonos.* Were we to offer an opinion without trying the experiment, we should doubt these consequences of mechanically irritating the different parts of the spinal cord. Baron Larrey, we know, long since supported the opinion, that injuries of the nerves on the anterior part of the body produced emprosthotonos, while those on the posterior were followed by opisthotonos. The answer to this is very simple;—wounds of the front of the body are much more frequent than those of the back, while emprosthotonos is an occurrence of so great rarity, that many surgeons doubt its existence altogether.

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* Phil. Trans. for 1815.

With regard to the cure, it appears to us to be a most essential point to separate the traumatic from the idiopathic species. The former is generally a rapidly fatal disease; the latter, so far from being, as Dr Reid describes it, a disease which "generally runs through its course with the utmost expedition," is often a much more mild and tractable affection, a fact well known to every planter in the West Indies, who never considers his negroes as safe when the disease supervenes on a wound, but is frequently successful in alleviating the idiopathic species. It is chiefly in these cases, and not where it occurs from wounds, that it is so apt to put on the chronic form. We shall not enter into an examination of particular remedies, as applied to either species; but we would ask any practitioner, when has he ever seen Madeira given so as "to cause intoxication?" For ourselves, we can safely assert that we have never seen an instance of a patient truly tetanic having been made drunk; nor have we ever known a single case where even an approximation to such a state has been produced by the most liberal, nay unbounded supplies of wine, brandy, and laudanum. Blisters we have often known applied to almost every accessible point of the body, without any relief; and as for sudorifics, the patients in the majority of cases have been in a constant profuse flow of perspiration of a most ammoniacal odour, which has produced no alleviation whatever of their symptoms. In truth, whenever we reflect upon what we have seen of tetanus, our hopes of a rational pathology, and of a successful practice, are sadly depressed.

We have left ourselves but little room to follow Dr Reid into his third chapter on Hydrophobia, which is, also, according to him, a spinal disease; and we the less regret it, as we found that we were not able to keep pace with his reasonings; for, with our utmost exertions, we "toiled after him in vain."

With regard to the treatment of hydrophobia, we must differ from the Doctor on the practicability and propriety of excision, which we conceive to be the most promising and easiest of all the modes of prevention. Of the powers of oxygen and acetous acid, which he recommends, we know nothing more than we do of at least an hundred other remedies proposed for the "cure of the bite of a mad dog." Of blood-letting, however, we happen to know the very important fact, that it has often relieved, and sometimes cured; although Dr Reid states, "that in no one instance is it asserted to have been attended with any advantage." In the 9th Volume of this Journal, however, (p. 22. *et sequent.*) he will find cases by Dr Berry, and by Dr Shoolbred of Calcutta, abundantly satisfactory on that point; and in the 11th Volume, (p. 413,) he will see references to several others.

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* Vide Experimental Inquiry into the Laws of the Vital Functions, by A. P. Wilson Philip, M. D. P. 105, 108, 148. London, 1817.

In the appendix is a case and dissection of a soldier, which the author thinks strongly in support of his opinions on the spinal origin of tetanus and hydrophobia. We cannot afford room to transcribe it, and must refer to the work itself.

We suspect that some of the opinions supported in the present work are not quite unknown to us, and that, like the good Vicar of Wakefield, "we have heard all this before;" but whether our knowledge has been derived from reports of the lectures already alluded to, or from orations in the debating societies of the students of the university, we cannot say; we by no means, however, wish to deprive Dr Reid of the credit of having supported them with considerable boldness, and occasionally in a manner different from the ordinary mode in which we have heard them stated. Sometimes, indeed, his views have been presented to us in a way quite beyond our comprehension. This we might have tolerated; and we could have made ample allowance for an author pushing a cherished theory somewhat "à outrance," especially when we are satisfied that the basis of that theory is, in many points, sound; but we cannot reconcile to our sense of literary candour, the total silence which pervades the work, with regard to almost all that has been done upon the subject of spinal physiology and pathology, by contemporary individuals.* Whether this may have arisen from accident, from design, or from defect of information, it is not for us to decide; but we may venture to predict, that the future fame of Dr Reid would have suffered no diminution by a recognition of the labours of others.

We confess that we have not derived much satisfaction or instruction from this essay, and certainly infinitely less than we anticipated from its first pages. We have no hesitation, however, in saying that its author is, in our opinion, an acute man, whose talents, if directed to points of pathology, and not warped by a favourite theory, may yet effect something much superior.

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* Mr Brodie is once named in the first chapter, but neither Dr Wilson Philip, Mr Clift, nor Le Gallois, are mentioned throughout the book, and the existence of the Edinburgh Lecturer is not even hinted at.

We have already stated it as our opinion, that much valuable information may be gained, by having more frequent recourse to examinations of the state of the spine. The difficulty of performing these has deterred many surgeons from the attempt, and it therefore becomes an object of importance to strike out a suitable and easy mode of conducting the process. We have seen the spinal marrow laid bare, by sawing on each side of the spinous processes from behind; a much easier, more simple, and more effectual way, is to saw through the bodies of the vertebræ obliquely on each side from within. A saw, after the model of Mr Hay's, may be advantageously employed for the purpose, but somewhat stronger and larger. The preliminary steps need differ in nothing from the ordinary mode of opening the cavities, and when the spinal canal and its contents have been examined, the bone can be dropped into its place again, and no greater mutilation will be apparent externally than in dissections as commonly conducted,—a matter of considerable importance in private practice.

We are not so sanguine as to suppose, that, in every case where the spine may be suspected, we shall be able to discover either the cause of disease, or a clue to its removal. We know, that in some instances a morbid state of the nerves has been discovered from the point of lesion to their origins; but we know also that very frequently no morbid appearances whatever have been detected by the most expert anatomical investigator. Nor will this appear singular, when the extensive sympathy which exists between the different parts of the body is considered. Indeed, we often find that parts which suffer sympathetically are more severely affected than those in which the organic lesion exists. How often do we see this in severe attacks of dyspepsia, in which the most violent symptom is pain, and sense of tension, and fulness in the head, while scarcely any morbid sensation is referred to the stomach itself. Blows on this organ often occasion fatal apoplexy, and blows on the head often produce their most immediate and distressing effects on the stomach. Frequently, indeed, we find severe injuries of one of these organs without any sympathetic affection being exhibited by the other; but we should not be discouraged by these and other seemingly inexplicable anomalies. Much has been done by ingenious and scientific men who have placed the reins of fancy under strict control; much, it is true, is still left unexplained, but the results of the past afford us strong assurance of the ultimate success of future investigations. Le Gallois in France, and Dr Philip in our own island, have set before us most brilliant examples of patient and truly philosophic industry. By experiments ingeniously conceived and ably executed, they have removed many of those apparently insurmountable obstacles which perplexed the path of truth, and their labours, if duly appreciated, may enable less acute, and less original inquirers to explore it to its termination.

We had one additional observation to make to the junior classes of our readers, who may be inclined to prosecute the inquiry into the connection which may exist between the state of the spine, and that horrible and obscure disease which has formed the principal part of our critical analysis; we had intended to warn them against the error of overlooking all other causes except that favourite one which either the peculiar tenets of their early teachers, or some accidental circumstances, connected with their own practice or their speculative opinions, may have led them to adopt, but we find our ideas so admirably expressed by a well known philosopher, that we shall rather avail ourselves of his words, than run the hazard of enfeebling the remark by employing any language of our own: "Ces sortes de prejuges étant les premières impressions que nous ayons eprouvées, ils ne manquent pas, de nous paraitre des principes incontestables." Condillac, Essai sur l'origine des Connoisances Humaines.

III.

Observations on Contagion, 2d Edition, corrected and enlarged by original Communications. By WHITLEY STOKES, M. D. Honorary Fellow of the College of Physicians, Lecturer on Natural History to the University of Dublin, late senior Fellow of Trinity College, and late Professor of the Practice of Medicine. Dublin, Hodges and M'Arthur. 1818. pp. 67.

THE opinions, we believe, of all well-informed persons are pretty nearly settled as to the contagious nature of typhus fever. But it is of such vast importance that the public at large should also entertain correct notions upon this subject, and every production which puts the question into a new and forcible light, renders good service to society. In this view, Dr Stokes's observations are very valuable: they must have produced the best effect in Dublin, where his publications first appeared; and they furnish, even to the most learned of the medical profession, some original and important matter.

"By contagion," says the author, "I would denote a substance, produced on the body of an animal diseased, which will, if applied in considerable quantity, to the body of a healthy animal of the same species, give rise to similar disease." The existence of such a substance in small-pox, measles, the venereal disease, &c. cannot be doubted, and the bearing of Dr Stokes's argument, in the commencement of the tract before us, is explained in the following passage.

> "The objections which have been relied upon against the existence of contagion in certain fevers, will lose much of their force, if we can shew that similar circumstances are observed, though in a less degree, in those diseases which all acknowledge to be contagious; and the laws of contagion, which may appear doubtful when we attempt to deduce them from one or two species of diseases, appear to assume increasing regularity as our views extend." p. 2.

The first part of the book, accordingly, is a review of the proofs, that plague is contagious; a fact which the author conceives to strengthen forcibly the reasoning as to typhus, from the analogy which exists between these two distempers. Upon this subject he brings forward some interesting statements, from the experience of Mr West, now surgeon of the 27th regiment, during the prevalence of the plague at Rosetta, which it is possible we may refer to on some future occasion; but as they relate more properly to the treatment of the plague than to the question of contagion, we shall for the present pass them over, observing only, that we concur in the concluding remarks of Dr Stokes, that "the conduct and escape of this party was creditable in the highest degree to the firmness and industry of all who were engaged in it, and especially to Mr West, who had the medical command."

As we have never seen a case of plague, we express any opinion respecting the nature of that malady with considerable hesitation. But, so far as our knowledge does extend, we cannot concur with Dr Stokes, in regarding it as identical with typhus. "We are wrong," he says, "in flattering ourselves, that the circumstances of society are so altered, that we can never again be visited by the plague. The truth may be, that we have become inured to *a milder state of* its contagion." p. 17. Now, though we admit that typhus, in some of its characters, resembles plague, as it does also certain forms of small-pox, scarlet-fever, and measles; we still consider the buboes and carbuncles which present themselves in every well marked case of plague, and the extreme rapidity of its progress in the great majority of fatal cases, as sufficient nosological distinctions. If buboes or carbuncles were usual even in the worst forms of our typhus fever, the identity, which Dr Stokes supposes, might be contended for. But, without such proof, it seems to us the safer course in practice, and the sounder as to scientific arrangement, to name and regard the two diseases as distinct.

The extension of typhus through families is one of the direct arguments for the existence of contagion, in that malady, which Dr Stokes illustrates with the greatest success, and his reasoning upon this point appears to us to be original, as well as correct.

"There is," he states, "in typhus, a certain degree of grouping which cannot be fairly referred to mere casualty; and this assertion depends on the same principles of the *doctrine of chances*, as those which prove that the odds against throwing the same cast of the dice many times successively are enormous." p. 18.

A certain family, consisting of twelve persons, resided in Dublin during the prevalence of the fever, which still rages in this city. In the general course of the epidemic in the district in which they reside, one person out of seven took the fever, but eleven individuals of this family were subjects of it.

"Had the family consisted of fourteen it might have been said before the epidemic, that if two of that family sickened, they bore only the average share of the general calamity: In the family of twelve, the sickening of two would have been more than their average share, and the sickening of eleven, so much more, that the chance against that event would have been nearly 189,600,000 to 1."* p. 18-23.

The author next mentions several facts, which concur with the numerous statements already given in our volumes, in establishing, not only the propagation of fever by contagion, in the houses and families of the lower classes, but the danger which necessarily arises to the highest orders of society, from the extension of the disease by unavoidable intercourse. In one instance, in Dublin, in 1800, a poor family, fully infected with the worst form of typhus, were found employed in making up liveries for the servants of the Lord Lieutenant of that day.

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* "I proposed," says Dr Stokes, in a note, "the following problems to a friend particularly acquainted with this species of computation: 1st, An epidemic prevails, so severe that one of seven sickens. A family of twelve is selected in a particular district, before the epidemic has visited it. What is the chance, that eleven out of that family should take the disease, supposing the sickening of one of the family does not promote the sickness of another, and supposing the family not unusually liable to disease?" The answer is, that the probability against the event is as the numbers we have stated in the text, "*if the population amount to* 7000!" "2d, The same conditions being assumed, what is the chance, that *in any family* of twelve, within the district, eleven will sicken? Answer, it is above 300,000 to 1, that no family of twelve will have eleven sick." (p. 23, 24.) In quoting this statement. we take it for granted that the numbers are correct; but any error in this respect will not invalidate the *argument*, which is equally ingenious and convincing.

The objections against the contagious origin of typhus, which Dr Stokes refutes, are: 1st, The escapes of infants, whose mothers die of the disease, and reciprocally; an argument which, he shows, might be applied also to disprove the existence of contagion in small-pox and the plague. 2d, The length of time which typhus requires to spread through large families. 3d, It has by some been argued, very injudiciously, that the admission of concurrent causes, such as want, cold, damp, &c., is in fact a confession that *contagion* does not contribute to the malady. 4th, The rare occurrence of fever in opulent families is supposed by some persons to prove that the disease is not contagious; but this rarity is by no means so great as has been asserted. Every individual of an opulent family, in a town within a few miles of Dublin, was successively affected; although the epidemic in general, at that time, assailed only one person in thirty.

> "The most probable opinion," says the author, "respecting the contagious nature of our typhus, appears to me to be this, that it is contagious in a slight degree; but that its contagion is more manifest in the families of the poor, because they are more exposed to the exciting causes of the disease, such as filth, damp, cold, and famine, and often sleep in the same bed with the sick." p. 26.

5. The opinion that physicians, nurses, clergymen, and others who nearly approach the sick, escape this malady, has no better foundation; for it is notorious that such persons frequently suffer from it; * nor would their exemption apply as an objection to contagion in typhus only, since, with due precaution, the plague itself has frequently been escaped from. As in the case of Mr West's party, above mentioned, who remained four months shut up in a peste-hospital, with a succession of patients, many of whom presented the most horrid forms of plague, without any one of the party taking the disease: And in the army of Sir David Baird, during their expedition from India to join the British troops in Egypt, such was the admirable success of the precautionary measures, that the loss from plague was only 203, out of an army consisting of near 8000

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* This has been, unfortunately, but too certainly established, during the present epidemic. Almost every clerk, nurse, and resident servant in the hospitals of this city have been infected.

effective men, who remained for fifteen months in an infected country.

The Laws *of contagion* which Dr Stokes lays down, as "sufficiently constant to deserve attention, though not perfectly regular," are, 1. That it is conveyed either by immediate contact or proximity, or by fomites. He quotes the experiments of Dr O'Ryan at Lyons, and the success of the precautions used by Dr Russell, in prescribing during the plague at Aleppo, to prove, that dry air cannot convey contagion to the distance of many feet; and he adds, justly, that "the knowledge of this property of contagion is of the greatest consequence to the interests of humanity, as it may encourage the benevolent" (and in the same proportion it may calm their apprehensions for the safety of their friends) "to afford their assistance, by their advice or directions, in situations in which ignorance of this principle might have deterred them from acting. 2. Contagion seldom produces its effects immediately, the *latent period* being of very different duration in different contagions, and in different states of the same contagion. This latent interval, however, in most diseases, is not a period of perfect health; in lues, a depression of spirits, and a disposition to general perspiration; and in typhus, terrific dreams and depression are observed, before the characteristic symptoms of these maladies are fully evolved.

> "In order to determine the latent period of any disease, we should endeavour to collect as many cases as possible, in which the patient had been exposed to contagion for very short periods; the sum of all the observed intervals between the reception of contagion and the accession of disease will give the average of the latent period, as in any other case in which average quantities are sought. But if such cases are not to be collected in sufficient number, it will answer tolerably to take cases in which the exposure had continued for a few days, and, reckoning from the middle point of time between the commencement and conclusion of the exposure, to the accession of the disease, as the latent period of each case, take the average as before. I should not have stated principles of approximation so obvious as these are, but that they have been unaccountably overlooked by some writers on contagion, who possess, and even deserve celebrity. Although we could select a considerable number of cases, in which the patient was exposed to contagion merely for a moment, there would still remain many sources of error, which are best avoided by taking the average of several observations. The operation of exciting causes seems to be the principal of those sources of error, as they seem capable of shortening the latent period." p. 33.

3. A sufficient dose of contagion is requisite for the effect, and more than is requisite appears to render the disease more

severe. 4. Many individuals appear to be exempted from the influence of particular contagions. 5. The effects seem to be less severe when the contagion is gradually communicated. 6. Many contagions seem scarcely ever to affect the human constitution more than once. 7. The part of the body infected modifies, in some cases, the subsequent disease, as in lues, and less distinctly in variola. 8. Different contagions sometimes combine their effects, and sometimes interfere with each other. 9. Many contagions are confined in their agency to one species of animals; others pass from one species to another.

The means of preventing or mitigating the effects of contagion are stated by Dr Stokes under four heads: 1. The destruction of the matter of contagion, and the expulsion or separation of it from infected substances. On this part of the subject, very good directions for fumigation are given; but the author is of opinion, and we entirely agree with him, that our reliance on the efficacy of this process is, in a great measure, traditional, and that experiments are wanting to prove its power with the accuracy of science. * He afterwards adds, we think very happily, in speaking again of fumigations:—

> "They were part of a process which did succeed; and it may be said, until we can separate the wheat from the chaff, it is not wise to throw out either. Time, however, may be lost in teaching the population of a great city to purify themselves or their clothes by nitric acid, or chlorine, when the business might be as effectually done by soap and water. Mankind are too much disposed to seek for wonders, when their feelings are aroused, and to neglect the safe and simple resources that nature and reason point out. We require the admonition that Naaman received from his servant, 'If the prophet had bid thee do some great thing, wouldst thou not have done it? How much more, when he saith unto thee, 'Wash, and be clean?'" p. 46.

2. The separating of infected from uninfected persons. On this point, Dr Stokes's observations are principally applicable to Dublin, a capital which affords great facility, from local circumstances, for cutting off communication with the adjoining country; and which is rich in admirable public institutions for the sick. He mentions, however, that the good effects of the hospitals in Dublin were accompanied with one very formidable source of danger, from the introduction of infected articles into the metropolis, along with the sick; and, to prevent this mischief, he proposes to encircle the town with twelve fever hospi-

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* We should perhaps except those conducted at Torgau, by Richter, under the superintendence of Graefe. Ed. Med. Journal, Vol. XIV. p. 376.

tals, on the smallest scale; five cottages, connected by clean gravel walks, being sufficient, probably, in each case, to furnish this important accommodation. The remaining measures which he recommends are excellent in their design, and, no doubt, would be effective; but, we fear that some of them would not be very easy of attainment; requiring, as he himself acknowledges, great expence, high authority, much integrity, vigilance, and cordial co-operation of all ranks of people,—a concurrence which it has never yet been our good fortune to observe in any public measure; and which, perhaps, our northern temperament prevents our being sufficiently sanguine to expect, during the remaining course of our experience. We shall not follow the author through the pages in which he discusses measures for checking the progress of the Plague, if it should appear in Dublin, because we really consider this as not a very probable occurrence; nor, if it should unfortunately take place, do we quite agree with him, as to some of the measures which he recommends for obviating its progress; but there is considerable force, we think, as well as good feeling, in the following passage.

"Mendicity is a great cause of disseminating contagion. This has been an observation only too well established in various epidemics. This subject should be speedily attended to. It seems inconsistent to lay vessels from Charlestown under quarantine, and keep the avenues of the city open to beggars, who flock from all the infected districts of our own country. We should, however, enter on this difficult question, with a firm determination to let no inconvenience, no danger, drive us from the sacred principles of justice and mercy. If we desert these, God and man will desert us. I cannot see on what principle mendicity is treated as a crime. I admit it is a great inconvenience,—at present the source of great dangers. Still these do not make it a crime. On the contrary, I am of opinion, it may be a man's sacred bounden duty, if he cannot support his children, to beg for them. If, for your own safety, you deprive an unhappy man of his last resource, you are bound, by every tie of justice and compassion, to give him some compensation." Pp. 56.

The book concludes with extracts from the correspondence of the author in Ireland, which confirm his opinions, and afford some very interesting facts respecting the present epidemic. Dr Morison of Newry states, that he had treated his patients ill of fever with mercury; and that, *in one hundred cases, none died in whom ptyalism was excited.* And this gentleman expresses his opinion, that the co-existence of the febrile and mercurial actions is, in almost every instance, incompatible.

"The cases were so far selected, that the bad ones were always the subjects for the mercurial treatment; and in those where local inflammation seemed evidently to have existed, the use of mercury was particularly apparent. In none was this treatment begun with after the eleventh day of fever; but in many instances the disease was cut short by the sixth or seventh. Ptyalism was difficult to be brought about, in proportion to the violence of the disease. The submuriate of mercury, in doses of two or three grains, three times a-day, (often with opium,) was the form generally made use of." Pp. 66, 67.

An interesting statement, also, is given by Dr Rogan, respecting the fever which appeared at Strabane in 1817, an enlarged account of which formed the substance of that gentleman's inaugural dissertation. * The population of that place is about 4000 persons, of whom more than one-seventh were the subjects of fever, within the last five months of 1817; and the system adopted there, to check the progress of the malady, does great credit to the liberality of the late Marquis of Abercorn, and the good sense of all the inhabitants.

We shall close this article with some extracts of a letter to Dr Stokes from Dr M'Donnell of Belfast; a practitioner with whose skill and various acquirements we are well acquainted. "I have always," says the writer, "kept this question (as to the contagious origin of typhus) in view; and can safely say, that the evidence, not only of its propagation by infection, but by that solely, has always appeared to me as clear, as that measles, scarlatina, or pertussis, were infectious." This opinion is supported by some very decisive facts observed at Belfast, by Dr M'Donnell; but we had rather conclude with the following statement of what occurred within his observation at Rathlin, because it seems to us to unite in itself every thing that is necessary to render an experiment in physics unobjectionable and decisive. The place alluded to is an island, containing about 1200 inhabitants, four miles from the north coast of Ireland.

"In August 1814," says Dr M'Donnell, "when there was little or no fever on the coast of Antrim, I went to Rathlin, and was surprised to find two families, about the centre of the island, in typhus. In the first house, I inquired in vain as to the origin of the fever; but, on visiting the second house, I discovered, that some of the connections of the family had died in Greenock; they had gone there to visit the sick, and had brought home the clothes of those who died at Greenock in a chest, and had taken the disorder soon after their return. Fortunately for the people, it happens that the proprietor, the rector, the agent, and the priest, are always resident. I had a conversation with them all on the subject, and explained to them my views on the treatment and prevention of the complaint, which soon

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* Edin. Med. Journal, Vol. XIV. p. 542.

ceased, but not until several were affected, and three or four died. Since that period until the present date, (January 1818,) no case of typhus whatever has occurred on that island; although it has been very prevalent, as every one knows, on every part of the coast opposite Rathlin.——Now, it cannot be said, that the exemption of this island arises from the inhabitants being of a different description, being less exposed to cold, heat, fatigue, hunger, bad food, filth, moisture, confined air, and the long list of causes supposed capable of producing this dreadful distemper. Their exemption arises from the perfect conviction the people now have, that the distemper is portable, and that it was imported in 1814. This has been often explained to them from the pulpit, although few of them can speak English, and of these only a very few can read, yet they understand this matter, and act on it sagaciously. They provide for all their own poor; and if itinerant mendicants arrive from Ireland or Scotland, they instantly supply them with food, and send them back; and a native of the island, who crossed the channel in November last to attend a relative in fever, has not been suffered to return." Pp. 60, 61.

IV.

Lettere del Professorè SCARPA *al* Professore MAUNOIR, *concernenti l'Opera del* Signior ADAMS *sulla Cateratta, e sulla pupilla artificiale.* 8vo. Milano, 1818.

(Concluded from No. 58, p. 142.)

LETTER III.

Pavia, 10th December 1818.

ALTHOUGH, as I have shown in my preceding letter, the three reasons do not exist, on account of which Sir William Adams disapproves of the depression of the solid cataract to the bottom of the eye; and from which he concludes, that the depression of the *solid* lens is uncertain in its event, and frequently hurtful, yet the new operation proposed by him for the cure of this species of cataract may, in some respects, be advantageous, at least in some particular circumstances. Whether this is the case, will appear from the following short analysis of the operation.

"In old age," says Sir William Adams, "when the nucleus of the lens is frequently so dense and hard, that the needle cannot be passed directly through its centre, with a knife of the same form as that used for artificial pupil, but somewhat smaller in size, I slice off, by repeated cuts, as much as I can of the opaque crystalline and its capsule, taking care not to dislocate the former body, nor to separate the latter from the ciliary process, (ciliary circle.) The fragments being thus placed in the anterior chamber, leaves a space between the remaining anterior part of the cataract, and the posterior part of the iris, which becomes occupied by aqueous humour. This not only acts on, and softens the remainder of the solid lens, but also materially tends to prevent it from pressing against the iris, so as to excite inflammation, which, from being deprived of the support of the anterior part of the capsule, it would be otherwise likely to do. If the nucleus of the cataract be not very hard, nor of an unusually large size, when the portions placed in the anterior chamber are absorbed, it may then, by a second operation, be wholly cut in pieces, and brought forward for the purpose of effecting a more speedy absorption. Should the surgeon, however, on trial, find, from its size and solidity, that he is unable to accomplish this, he should again slice off as much as is in his power of the softened cataract, and, in a similar manner, place the fragments in the anterior chamber, when there is no doubt, that, by a third operation, the nucleus will admit of being cut in pieces and placed in the anterior chamber, where it will soon be absorbed. A long time must not be permitted to elapse between the operations, as the circumference of the lens, being much softer than the centre, it will first become dissolved, when the nucleus, from its diminished size, may slip undivided into the anterior chamber where, by its mechanical friction against the iris, it will keep up a distressing degree of pain and irritation in the eye. This ought, therefore, to be avoided, by repeating the operations in proper time. If, however, this accident should occur, I would recommend that a small puncture be made in the cornea, for the purpose of extracting it." (*Practical Observations on Diseases of the Eye*, p. 144.) The author, in his recent work on this subject, adds, (p. 268,) that when the cataract is of such a degree of hardness, that it cannot be sliced with the needle nor with the small knife, it is proper to push it entire through the pupil into the anterior chamber, (provided, he says, that the pupil be sufficiently dilated,) to be then extracted by means of an opening of the cornea, proportioned to the size of the lens.

Considering attentively this description, it follows, in the first place, that it is incumbent on Sir William Adams, and all those who would wish to follow his practice of curing *solid* cataract by breaking it into pieces, and pushing the fragments into the anterior chamber, to know, previous to the operation, the degree of hardness of the opaque lens, not to be obliged, if it were too hard, to withdraw the double-edged needle, and introduce the small knife similar to that used for the division of the iris.

On this point Sir William Adams accuses me (p. 26) for having said ingenuously, that, except in congenital cataract, which I knew was always *membranous*, or for the most part so, and the *secondary* cataract after depression or extraction of the crystalline, which, I was aware, was generally *capsular*, with regard to all the other species, I was unable to predict with certainty of what consistence a crystalline cataract was before operating. Anxious to be instructed in this point by Sir William, I have not found on this point any other information than "that an oculist of just and accurate observation will rarely be deceived in his opinion of the nature (degree of consistence of the cataract) upon which he is about to operate, although he is unable to give any verbal or written description that can convey an accurate idea of the various kinds and shades of cataract." Fortunately this exquisite knowledge is not absolutely necessary for me; for having introduced my *curved* needle, if the opaque lens is *fluid*, *soft*, or *caseous*, I break it easily by merely pushing it towards the pupil and into the anterior chamber; if it is *hard* and *resistent*, and obedient to the pressure of the needle, I depress it into the vitreous humour and to the bottom of the eye. The young operator, says Sir William Adams, who is unable to predict the degree of consistence of the cataract to be operated on, will proceed cautiously, (p. 261;) that is, before preparing to cut the lens into pieces, he will act with the point of the needle to ascertain the consistence of the lens. But I observe, that, if the young operator shall find the cataract resist the division into pieces by means of the double-edged needle, he will be obliged to withdraw it, to substitute for it the small knife similar to that for the division of the iris.

Examine now, with me, the series of operations which it is necessary to make, according to the directions of our author, when we meet with cataract, *one-half* only of the nucleus of which is *solid*, and when the lens is so *hard* as to render abortive every attempt for breaking it into pieces with the double-edged needle or the small knife. In the first case, the surgeon can only pare off its soft circumference, and leave the hard nucleus *in situ*, that, in proportion as it becomes softened by the action of the aqueous humour in the posterior chamber, its surface may admit of being cut; and, finally, that after repeated introductions of the needle, the whole, reduced into small pieces, may be pushed into the anterior chamber. If we wish to avoid these repeated introductions of the needle into the eye, and free the patient from the anxiety and uncertainty in which he is placed with regard to the event of the operation, it is indispensable to push at once, besides the soft outside, the hard nucleus of the lens also into the anterior chamber to be immediately extracted by an incision of the cornea. Two or more operations, therefore, are unavoidable, whether we wish to diminish the size of the nucleus by cutting it *in situ*, or to extract it from the anterior chamber by the incision of the cornea. If the lens is so hard in its whole circumference that it cannot be in any way divided into pieces, the two operations are equally necessary; under the condition, besides, (which is not always the case,) that the pupil be sufficiently dilated to allow an easy passage to the entire lens, and without the iris being injured by the excessive stretching.

As long as Sir William Adams confines himself, in his recent work, to compare his new method of curing *solid* cataract with the common manner of extracting the opaque crystalline by the incision of the cornea, he has plausible reasons for preferring his mode of operating to *extraction*, * such as is usually practised; for, by following his *new* method, every sort of pressure on the eyeball in making the lens pass from the posterior to the anterior chamber is undoubtedly avoided, and the discharge of the vitreous humour is with certainty prevented. But these reasons, with regard to *solid* cataract, have not the same importance when we compare the *new* operation with depression of the crystalline, which resists the point and pressure of the needle. There is nothing more evident, to any one who has not been frightened by the horrible picture of the symptoms occasioned by the pressure of the *solid* crystalline upon the soft retina, than the following statement: Having passed the point of the *curved* needle through the anterior capsule, and then into the solid substance of the lens, and made a movement from above downwards, and from before backwards, the anterior and posterior capsules are ruptured, and, along with the crystalline, the fragment of capsule detached by the curved needle, is in an instant conveyed with facility out of the axis of vision, and immersed in the intimate texture of the vitreous humour, leaving the pupil free from every opacity. The patient, after this easy and quick operation, immediately enjoys the fruit of it. And here I stop; for, in my opinion, to say more in proof of the simplicity and utility of this practice in opposition to that of the two operations by the puncture of the sclerotic coat, and the opening of the cornea for the cure of *solid* cataract, would be an abuse of your friendly condescension, especially when we consider that the three reasons adopted by the author against the depression of the *solid* lens are of no importance.

It is in vain for Sir William Adams to assert, that, in spite of the repeated introduction of his instruments into the eye, and notwithstanding all the movements which he is obliged to make for paring and dividing into pieces a *solid* cataract, until he has succeeded in making it pass in an entire or divided state into the anterior chamber; or, in other words, notwithstanding the two or more operations, that is, one certainly, or more, with the needle, and another consisting of the incision of the cornea, proportioned to the fragments to be extracted, he has no severe consecutive symptoms to combat. Daily experience teaches us, that the severity of the subsequent symptoms to any operation whatever, are always in proportion to the severity of the irritation produced by the operation; more especially when the operation is performed on an organ so delicate as the eye. And that the symptoms consecutive to the *new* operation for the cure of *solid* cataract are not so slight, may be inferred, both from analogy and from the consideration of the active means which Sir William Adams employs for subduing them, (*Practical Observations*, p. 149;) such as copious evacuations of blood from the arm, the opening of the temporal artery, leeches to the internal canthus of the eye, anodyne fomentations, the tincture of digitalis internally in doses of twelve drops every two hours, until it produces nausea, and other powerful remedies, for which I do not recollect ever to have had occasion after the depression of a solid cataract, on account, without doubt, of the easy execution of this operation, and the very slight irritation done to the eye.

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* We have, in the course of this Journal, repeatedly expressed our sentiments on the merits of extraction; and whatever may be our opinion of it compared with depression, we are not disposed to yield any pre-eminence to the author's *new* method. Extraction, skilfully performed, is very rarely followed by those frightful consequences he has represented.—*Editor.*

You may read on this point (*Bibliothek für Ophthalmologie* of *Himly, Vol. I. P. I. page* 47) the following account of the new operation, given by Dr Günther. "If it happens (says he) that Sir William Adams cannot cut a *solid* cataract in pieces, he pares off as much as he can of the substance of the lens; and repeats this operation according as the lens itself is softened. But the great friction and working with the needle cause an inflammation more than moderate to supervene afterwards, although Sir William refuses to confess it, but which, nevertheless, happened in all the operations which he performed in my presence." At least we must agree, that, in the mode of operating on *solid* cataract proposed by Sir William Adams, there are not the conditions which Celsus wishes, *ciio, tuto, et jucunde.*

It will not excite any surprise in you, if, in so great a difference of opinion, Sir William Adams does not find my *curved* needle to his taste, although, in certain circumstances of *capsular* cataract, he employs, with advantage, a needle bent at the point, not unlike to mine, (p. 264.) I have reason to doubt whether he is really acquainted with the original form and fineness of my needle, which he supposes to be *blunt*, while it is very sharp, as much so as the point of a lancet; but I agree with our author that he is right in saying that my needle is not proper for the operation of *solid* cataract, according to his *new* method; for he has occasion for a needle adapted for cutting in pieces a *hard* lens, and I only have occasion for a needle which can lay hold with its point of a solid consistent crystalline, and hold it firmly until, after rupturing the hyaloid membrane, I can plunge it into the substance of the vitreous humour out of the axis of vision.

With regard to the *capsular* cataract, I cannot well decide whether it is more advantageous to pierce the opaque capsule with the sharp *curved* point of my needle, to lacerate and detach it from the ciliary circle, or to cut it in various directions with a double-edged needle. I can, however, assert, that, when a portion of capsule loses the support of the lens behind it, as happens in the case of *secondary capsular* cataract, it is better to scratch or tear it with the point of the curved needle than to cut it; for, on attempting to divide it, it slips before the cutting edge, and renders every attempt to cut it in pieces, or to detach it from the ciliary circle, of no avail.

It is deserving of your attention in the whole of this matter, to remark the manner in which Sir W. Adams has kept his manner of operating subordinate to his favourite hypothesis of the frequent complication of cataract with fusion of the vitreous humour. He considers as a consequence of this complication, that is, of fusion of the vitreous humour with cataract, the easy detachment of the capsule, and consequently of the lens, from the ciliary circle, (p. 104.) And as he is very much afraid when it exists, according to his opinion of this complication of disease, that the one or other of these parts, on the needle being rested on them, will be detached, and sink into the bottom of the eye, from which they cannot be withdrawn, and therefore become causes of very severe symptoms. He therefore recommends, in this complication of the fusion of the vitreous humour and cataract, not to detach all at once the capsule in its whole circumference, but to leave a portion of it like a bridle, to retain the capsule in its proper place, until both of these parts are cut into pieces, and until the lens is turned before the needle, and pushed entire, or in part, through the pupil into the anterior chamber. This easy separation of the whole membranous bag of the lens from the ciliary circle, on the slightest touch of the needle, is considered by our author, in conformity with his theory, as an occurrence as frequent as he thinks the disorganization of the vitreous humour, which I have shown is very far from being a certain and determined fact. I, on the contrary, have very rarely met with this easy separation, and have never seen it followed by bad consequences, the capsule of the lens having yielded with unusual facility, in order to be removed from the axis of vision, and immersed in the vitreous humour.

From this concise comparison of the new with the old operation, there do not appear to be sufficient grounds for preferring the former to the latter, whenever it is not demonstrated by means of certain and numerous facts, that there is a dissolution of the vitreous humour, especially in persons who are advanced in years. And no one, I imagine, on the gratuitous assertion of the frequent dissolution of the vitreous humour, would immediately reject a successful operation easily performed, and successful, in comparison with two, three, or more operations by means of the needle or knife, in order to obtain the same end; and still more, only after several weeks from the time of the operation, and running the risk of violent consecutive symptoms. The only case, in my opinion, in which the breaking the *solid* lens into pieces may not only be advantageous, but even necessary, is that in which it has become *adherent* to the posterior surface of the iris; with regard to which point I shall delay speaking till my next letter. With regard to the records of the success, as a proof of the propriety and utility of the *new* operation, this is a delicate point, on which I abstain from touching at present.—I am, &c.

(To be concluded in the next.)

PART III.

MEDICAL INTELLIGENCE.

REPORT of DISEASES treated at the NEW TOWN DISPENSARY, *for the Quarters ending December* 1*st* 1818, *and March* 1*st* 1819.

	Aut.	Wint.
Febris continua -	203	202
——— intermittens -	3	1
——— ephemera -	5	35
——— infantum remittens - -	30	57
——— dentitionis -	22	12
Hydrocephalus -	3	1
Cephalæa et vertigo -	46	33
Apoplexia - -		2
Paralysis - -	6	7
Mania - -	1	1
Fatuitas - -	2	
Epilepsia - -	2	4
Convulsio - -	1	3
Concussio cerebri	1	
Ophthalmia et sequelæ	58	45
Amaurosis - -	1	1
Surditas - -	6	33
Otalgia - -	2	3
Epistaxis - -		2
Polypus nasi -	1	1
Odontalgia - -	20	21
Cynanche tonsillaris	30	13
——— trachealis	5	2
——— laryngea	1	1
——— parotidœa	2	1
Catarrhus -	73	15
Pneumonia - -	18	36
Pleurodyne - -	45	36
Pericarditis -		1
Hæmoptysis et Phthisis	21	28
Hydrothorax -	2	1
Palpitatio - -	9	2
Aneurisma aortæ	1	
Pertussis -	27	39
Hepatitis (acuta et chronica) - -	10	10
Icterus - -	6	4
Dyspepsia et Hypochondriasis - -	82	47
Hysteria - -	9	15
Vomitus - -	9	3
Hæmatemesis -	2	
Enteritis - -	6	6
Colica et Obstipatio	24	18
Diarrhœa - -	54	66
Cholera - -	2	
Dysenteria - -	4	9
Vermes - -	22	17
Tabes mesenterica -	4	6
Hæmorrhois - -	2	7
Prolapsus ani - -	1	1
Fistula ani - -		1
Hernia - -	2	1

	Aut.	Wint.
Dysuria	8	3
Nephralgia	1	1
Hydrocele	2	
Gonorrhœa	3	4
Inflammatio testis	4	3
Menorrhagia	7	6
Leucorrhœa	5	5
Cancer uteri		1
Amenorrhœa	8	8
Prolapsis uteri	1	1
Hydrops	17	21
——— ovarii		1
Physconia	1	1
Rheumatismus	53	43
Lumbago	9	11
Scrofula ossium et articulorum	5	4
——— glandularum	7	4
Morbus coxarius	1	1
Necrosis	1	1
Rachitis	2	1
Morbus spinalis	2	
Syphilis et Pseudosyphilis	12	14
Phlegmon	37	25
Anthrax	1	
Gangræna	1	3
Mastodynia	8	6
Paronychia	1	4
Erysipelas et Erythema	11	9
Pernio	1	1
Variola	10	20
Variola post vacciniam		6
Varicella	2	1
Rubeola	31	225
Scarlatina	20	15
Strophulus	2	3
Lichen	5	
Prurigo	2	2
Lepra	8	7
Psoriasis	8	11
Ecthyma	1	
Eczema	1	
Impetigo	17	12
Purpura	3	1
Roseola		1
Urticaria	1	3
Porrigo	22	23
Scabies	105	62
Herpes	14	8
Tumores	14	8
Ulcus	34	30
Ustio	13	12
Vulnus	28	10
Luxatio		1
Contusio et subluxatio	69	50
Fractura	1	4
Venæ varicosæ	6	
Fungus hæmatodes	1	
Asthenia	18	11

A summer and autumn of uncommon warmth (though not hurtfully dry, as in the southern part of the island) have been succeeded by an unusually mild, open winter, with winds generally westerly, sometimes very high, attended with heavy rains, and some falls of snow, but no frost of two days continuance.

It will be seen from the foregoing list, that the epidemic fever, which abated, as we have found it to do since the establishment of the Dispensary, in the summer months, has again increased in frequency since the autumn. The following table exhibits the result of our experience in a more precise form.

	No. of Fevers.	Whole Cases.	Proportion.
Quarter ending December 1st 1818,	203	1826	= 1 in 9
Quarter ending March 1st 1818,	202	2200	= 1 in 10.9

On comparing this with the tables formerly published in this Journal, (Vol. XIV. p. 123, and 654,) it appears that the proportion of fevers to other cases, has been as great during the last six months as we have ever seen it, excepting only during the three months of last winter, immediately preceding the opening of Queensberry hospital, during which time the Infirmary was unable to receive many of the fever patients who applied there. And as we believe that a greater number of persons have applied at once to the Infirmary when seized with fever, without first seeking aid from the Dispensary, during this winter than during last, it must appear, on the whole, that the disease cannot be said to be less prevalent among the poor than it was last winter.

The greatest exertions have been made by the managers of the Infirmary, to accommodate all the fever patients who have applied there, and we understand that the number of admissions of such patients there and at Queensberry House, has averaged above 160 in the month for several months past. We are also convinced, that the measures adopted by the committee of the inhabitants, appointed for the prevention of contagious fever, has been carried into effect with as much regularity and dispatch as the limited funds at their disposal, and the many difficulties and delays attending the execution of such measures, would admit, so that the present state of the epidemic, after all that has been done to arrest its progress, must be allowed to be by no means flattering to the "pride of art."

There are obviously various causes for the diffusion of the contagion, which still exist, notwithstanding all that has been done, and to most of which it is probably impossible to apply a remedy, but which are sufficient to explain the continuance of the disease, without inducing us to depart from the belief we formerly expressed, that contagion is the *exciting* cause of by far the greater number of the cases of fever now existing in Edinburgh. Some of the patients who have applied to the Infirmary, could not be received for want of room; many whom we have visited have been too young to be sent thither; many have obstinately refused to be removed from their homes;* some have gone to the hospital so late that they appeared evidently to have infected their attendants or neighbours before leaving home; some have come out so soon, that they have speedily relapsed, and not returned; in some instances, the cleaning of infected articles could not be accomplished so soon as was desirable; and in several there was good reason to think, that persons were infected by holding too much, or too close intercourse with their friends when in the hospital.

While it is obvious that such obstacles exist to the accomplishment of the end proposed, by the removal of the sick to hospital, and purifying of houses, clothes, and furniture, we must also admit, on the other hand, that when a manifest abatement of the epidemic shall occur, it will probably not be referable wholly to the efficacy of any such measures, but will be in part owing to a diminution in the contagious quality of the disease itself; such as, after a time, probably takes place to a greater or less degree in every instance where contagious fever is unusually prevalent; and such as has been observed, in a more unequivocal manner, and in a more marked degree, in the course of the more formidable epidemics, particularly of the plague.*

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* Of the 405 fever patients in the present report, more than a third were taken charge of at home.

In such attempts at the prevention, as well as in attempting the cure of disease, we must always look for much assistance from the beneficent provisions of nature; and as we never can say with certainty how many cases of contagious disease are prevented in the one case, or how many lives are saved in the other, we must be content with *probable evidence* of the efficacy both of our remedial and of our prophylactic measures. But although we cannot exactly *estimate the amount*, we can at least *establish the reality* of the good that is done, in both cases, by evidence much more than is sufficient to justify practice; and in regard to the efficacy of measures intended to check the diffusion of contagious fever, the experience even of a single dispensary furnishes evidence much stronger than can easily be obtained in favour of the usefulness of any particular remedy.

In the course of the last three years it has happened to us, some hundreds of times, to meet with cases of fever breaking out in poor families, often manifestly in consequence of communication with other families in which it had previously existed. In all such cases it has been our uniform practice to recommend the removal of the persons first seized to the hospital; and we have always assured the friends of the patients, when removal was practicable within the first few days of the disease, that if they were removed, and the usual means of purification employed, their families would escape; but that if they remained at home, other members of their families would take the disease; and these results, if not " constant and invariable sequences," have at least taken place so very generally, according to our predictions, that we are not aware of having forfeited any part of our reputation in any of the *closes*, by hazarding either the one or the other prognosis. We can point out several tenements in which patients have remained at home during fever in the course of the present epidemic, and in each of which from 15 to 40 other cases of the disease have taken place in succession. We should have little difficulty in pointing out above a hundred houses where a single case of fever has occurred, where the patient has been speedily removed and the place cleaned, and where there has been no recurrence. But we should hardly find five houses in all the closes of the Old Town, in which any patient in fever has lain during the whole, or even during half of his disease, and in which other cases of fever have not speedily shewn themselves. We believe that no medical man ever reflected on the results of his practice without having cause to regret, that the evidence in favour of the efficacy even of his best *remedies*, can seldom be rested on observation approaching so nearly to the nature of the *experimentum crucis.*

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* See Russell on the Plague, p. 202.

The symptoms of the disease have been very generally mild during the winter, particularly in young persons, and the mortality very small. A great number of the Dispensary patients have had a distinct crisis, by sweat, about the end of the first, or in the course of the second week of the disease. We have seen no petechiæ, and very little gangrene from pressure; but in severe cases, particularly in two fatal cases of old men, there was a great deal of tremor and subsultus tendinum, and delirium passing into coma.

It will be seen from our list, that small-pox likewise continues more prevalent than usual; and we have marked six cases of mild eruptive disease, as variola post vacciniam, which occurred in circumstances similar to those formerly detailed, (Vol. XIV. p. 389,) and presented the appearances of the modified small-pox there described. Three of the six were stated to have been vaccinated by, and afterwards shewn to, medical practitioners; a fourth was vaccinated by ourselves, but not till after exposure to the contagion of the small-pox. The marks on the arms of all appear perfect.

The symptoms in these cases, as well as in the cases described in our Report for July, differed in several respects, as was then stated, (Vol. XIV. p. 393-4,) from Heberden's description of chicken-pox. But it appeared in some cases which occurred in the practice of the Dispensary last summer and autumn, and to which Dr Thomson referred in his paper on the Varioloid Disease, &c. Vol. XIV. p. 518, that symptoms answering to the description of chicken-pox by Heberden and Willan, proceeded from the contagion of small-pox in some persons, protected by vaccination; and we are equally satisfied from our own observations, that in a few constitutions, *altogether unprotected,* the contagion of small-pox has produced symptoms equally answering to these descriptions of chicken-pox.

It is in this last way, as we are now convinced, that we ought to explain the circumstances of a case which occurred very early in the practice of the Dispensary, and is mentioned in Vol. XII. p. 247. At that time we saw several cases of mild eruptive disease in vaccinated children, and several of well-marked small-pox, in children never vaccinated, occurring together. We hesitated whether to consider the former disease as modified small-pox, or as chicken-pox accidentally prevalent in the same neighbourhood, until the case now alluded to occurred, which seemed to be decidedly chicken-pox, and the subject of which had never been vaccinated. This made us incline to the supposition, that two different contagions were there operating.

Five cases of the same kind have occurred this winter, in children from six months to three years old, who we were sure had been exposed to the contagion of fatal small-pox, who had not been exposed, as far as we could learn, to the contagion of any other eruptive complaint, and had never had cow-pox. Two of them were indeed vaccinated by us, at the time of their exposure to the contagion, but the vaccination failed. The eruption in them all was formed into thin flat scabs, and the inflammation about these was nearly gone about the fifth day. The eruptive fever was trifling. This agrees with what is related by Dr Thomson, as having been observed by him and Mr Gibson in regard to several of the patients at New Lanark, during the late epidemic there, who had never had either small-pox or cow-pox, and in whom the disease, seemingly proceeding from the contagion of small-pox, exhibited the character of chicken-pox, (Vol. XIV. p. 657.;) and one similar case is mentioned by Mr Hennen (Vol. XIV. p. 414) as having occurred in an infant three weeks old.

These observations should be taken in connection with the opinion of some of the older authors, noticed by Van Swieten, that the contagion of small-pox, when weakened in its virulence, particularly during the decline of epidemics, produces the chicken-pox.

Now, if the appearances of chicken-pox may be produced, in certain circumstances, by the contagion of small-pox, it does not seem *à priori* probable, though it is no doubt possible, that the same appearances are also produced by another specific contagion.

In most of the cases of small-pox that we have lately seen, as well as in those described by Dr Thomson, (Vol. XIV. p. 519,) the eruption has decidedly come out in successive crops; and the pustules that came out last, though they never became large, have taken their own time to maturate, so that, on the 12th and 13th days of the disease, when most of the pustules on the face were blackened and scabbing off, a few have appeared yellow and turgid.

We had an opportunity of learning lately the particulars of a case, where an eruption appeared, after slight fever, in a child never vaccinated, who had been exposed to infection from one of the Dispensary patients affected with modified small-pox. This eruption, from the report of the mother, was quickly " blown up into blisters," and was formed into scabs within six days. During this child's convalescence, several unvaccinated children in the same land, and exposed to infection from her, took small-pox, and two vaccinated ones took modified small-pox, and became patients of the Dispensary. Eight weeks after her first attack, this girl was attacked a second time with confluent small-pox, and died.

In two cases, we saw measles and small-pox co-existing. In the one, the eruption of measles came out on the fifth day of the eruption of small-pox, and both ran their usual course. In the other, the eruption of small-pox came out on the third day of the eruption of measles, which continued visible for two days more. In this case, the pustules of small-pox filled up more rapidly, and with a more pellucid fluid than usual, so as to be quite turgid and transparent on the fourth day; but in two days more the fluid in them became purulent. Both children recovered.

It will be seen from the list of diseases, that measles have prevailed almost as extensively among the poor this winter as they did in the winter 1816-17. There have been very few cases of the late, livid, and rapidly fading eruption with typhoid symptoms, which was described as frequent at that time, (Vol. XIII. p. 122,) and the mortality from the measles has been much less. In a few cases, the eruption, although florid, faded on the first or second day, without any typhoid symptoms, or other unpleasant consequences ensuing. Yet the disease has proved fatal in a considerable number of instances, both among the poor, and in the higher ranks. The mortality has been chiefly among children of feeble constitution, and among those who had previously suffered from hooping-cough, or long continued catarrhs. In the most weakly children it has come nearest to the malignant form formerly observed; and we are disposed to attribute a good deal of the difference between the two epidemics to the improved condition and better diet of the lower orders during the present winter.

Two or three very young children have died with spasmodic affections, approaching to convulsions, soon after the fading of the measles, without suffering much from dyspnœa; but in the majority of fatal cases, the breathing has been very short and difficult for some days before death, and coughing has appeared to give pain. The children have become nearly comatose, and the fatal event has been generally about the third or fourth day after the disappearance of the rash. In two cases of this kind, the appearances observed on dissection were, an inflamed state of the whole bronchia, with copious effusion into them, and a small portion of one lung in each case gorged with blood, so as to sink in water, with recent adhesion of the corresponding portion of pleura. One of these children had been bled, and the other twice leeched, immediately on the disappearance of the rash, without permanent relief to the dyspnœa. The only practical conclusion which suggested itself here was regret, not that the bleeding had been performed, but that it had not been performed *earlier.* The authority of Drs Willan and Bateman is adverse, in general, to bleeding early in measles, because oppression of respiration, with a labouring pulse on the first or second day of the eruption, usually disappear in the course of twenty-four hours; "but," adds Dr Bateman, "when the eruption has disappeared, and the cough and pains of the chest with difficulty of breathing become severe, bleeding and cupping may be repeatedly necessary." It has, however, appeared to us, that in severe cases of the disease, such as those now stated, bleeding after the disappearance of the eruption, to such an extent as the remaining strength of the little patients seems to justify, comes too late; and when the pneumonic symptoms at the height of the eruption are urgent, although we admit that there is no fixed proportion between their urgency at this time and their urgency afterwards, and although there is therefore a chance of their going off of themselves as the rash fades, yet, on the whole, we think that less is to be apprehended from immediate bleeding, than from a delay even of twenty-four hours.

In regard to blisters applied to young children, whether for measles or other affections of the chest, attended with febrile symptoms, it appears to us that considerable caution is required, on account of the ulceration and even sphacelus which we have repeatedly seen to result from them, even when they had been removed as soon as the vesications had risen. The management which we have found to answer best is to remove the blister after three or four hours, before any vesication has risen, and immediately apply a poultice. Under this application, vesications are often produced; but we have not seen any troublesome consequences from them, and even where the cuticle has not been raised, the cutaneous inflammation thus produced has appeared to be useful.

Midwifery Report from 1st *September* 1818 *to* 28th *February* 1819.

Number of Deliveries . .	134
Natural Labours	120
Preternatural, (breech presentations) . . .	2
Instrumental, (heads opened on account of deficiency of space in the outlet of the pelvis)	2
Complex, (twins)	2
——— (uterine hæmorrhage after delivery) . .	2
Premature and abortions	6

Letter from James Alexander, Esq. Assistant Surgeon 2d or Royal North British Dragoons, in reference to Dr Vetch and Sir William Adams. Belturbit, Ireland, 12*th December* 1818.

You will confer on me a very great favour, by inserting the following letter into your valuable work. The worthy purpose for which it is intended will be my excuse for troubling you. When we see genuine merit attacked, and wronged by the insidious machinations of an enemy, we are naturally awakened to a sense of its wrongs, and are inclined to give it the support justice demands.

I am led to this observation from the perusal of a pamphlet lately published by Sir William Adams, oculist, in reply to one by Dr Vetch, physician to the forces, on the subject of the granular state of the eyelids, consequent to purulent ophthalmia, and principally with a view to establish his pretensions to priority in describing this state of the disease, and promulgating the mode of cure now in general use. That Dr Vetch has been long acquainted with this termination of the complaint, and treated it in the most successful manner, is the point on which, I think, my evidence is required, and which I feel it my duty freely to give.

In the year 1811, I served as hospital mate under Dr Vetch at the Depôt, where I remained about five months, till the autumn of the same year, when I came to town to do duty at the York Hospital. During my stay at Bognor, the treatment of the granular state of the lining of the palpebræ, chiefly as connected with the opaque state of the cornea, formed a prominent part of the duty we had to perform. At that time the Doctor's practice was, as it had been for years before, to remove the rough lining of the lids either with the knife or scissors, but generally nothing more was requisite than the application of the argentum nitratum or sulphas cupri, having first everted the lids. This mode of practice is explained most clearly in Dr Vetch's publication of 1812, presented to the Army Medical Board in the month of March of that year. This pamphlet, Sir William most unequivocally in his late work denies the existence of, and asserts the ignorance of Dr Vetch on the point in question, and even goes so far, as to declare that he knew nothing of this state of the lids till the arrival of Dr O'Reilly, then hospital mate, at the Depôt. Now, it appears that I can speak to this point also; for in the years 1811-12, and posterior to the appearance of the pamphlet, Dr O'Reilly and myself were at the York Hospital, Chelsea, when Sir William Adams's attendance there was permitted, for the purpose of exhibiting, what he would have considered his new operation for the granular state of the palpebræ, but this we have seen was in fact, with a little modification, that which Dr Vetch had, for some years before, been in the habit of performing, and with a degree of success which does him honour. At this time (the beginning of the year 1812) I received from Dr Vetch a copy of the pamphlet in question, which I gave to my most intimate friends to read; it was in the hands, sooner or later, of many of the junior surgeons at York Hospital. Dr O'Reilly and myself were then intimate companions. He was resident hospital mate, and assisted Sir William in his operations; but it would appear by this statement, that not one of them gave him the least hint that he was anticipated by Dr Vetch. May I ask, whether he ever saw this pamphlet, or did he decline seeing it? Certain it is, that it was read by others at this period. Soon after, I joined my regiment, and *Dr O'Reilly was sent to Dr Vetch.* Even after this lapse of time, the circumstances here stated are as clear to my recollection as if they had occurred but yesterday.

I now come to this conclusion, that Dr Vetch treated the granular state of the lids, long before the career of Sir William Adams, on the same principles as now generally established, and of course he did so *before the arrival of Dr O'Reilly at the Bognor Depôt.* Sir William, in his pamphlet, has betrayed himself into an acknowledgment, that Dr Vetch knew that the granulations were formed in the lower eyelids but not in the upper, and he even grants that he describes their effects on the cornea. Can we imagine the Doctor to have been unacquainted with their existence in the upper, when he describes them so clearly in the lower lids? No, the fact is, he was aware of this circumstance, and directed his applications accordingly.

Dr Vetch, I can solemnly aver, never discharged any man as cured, before he ascertained, by repeated inspections, that the lining of the lids was quite smooth as in the natural state, and invariably regarded the destruction of the granulations as the *sine quâ non,* in forming a criterion of the perfect recovery of the eyes.

Thus, Sir, I have given a faithful account of what has come under my observation, and, if the evidence contained in it prove of any use to Dr Vetch, my purpose will be fully answered.

Report of the State of Fever in Newcastle-upon-Tyne, communicated by T. M'Whirter, M. D. Physician to the House of Recovery, Dispensary, and Lying-in-Hospital.

Fever has progressively increased, and has been more or less prevalent here during the last three years. In August, there had been admitted into the Fever Hospital several cases, some by families, of Typhus gravior. I now call it more certainly by the above appellation, because it has ever since characterized itself in the hospital by more malignant symptoms than those of the other patients; and because it has been readily traceable to the primary sources of contagion. It appears, then, that two kinds of fever have existed here; the one the Typhus gravior, demonstrably contagious; the other the Synochus, or Typhus mitior of Cullen. In the latter contagion has been only less marked than in the former. The poor have been the objects of the first; the domestics of the higher classes, and a few of their superiors, have suffered by the latter. The disease seems to have silently stolen round the town in its contagious form, and gradually to have sent radii from its circumference to the centre.

The superior attention of the higher ranks to the early removal of their servants to the fever hospital, with their own better accommodation, have probably modified the original disease with regard to them, so as to cause doubts, as to its contagious or non-contagious nature, among the faculty. In many instances, however, the milder form of the disease has sooner or later left its traces in the families where it has once appeared.

In perambulating this town as a dispensary physician, too many obvious causes of fever occur in the localities, and filth, and wretchedness of its poor inhabitants. One rather wonders that so many escape it, than that some are its victims. In former years, prior to the establishment of the Fever Hospital, it was never wanting, and appears to have annually affected many more individuals. Since its erection, about 14 years ago, the disease for which it was founded has never filled it till the last. It has ample accommodation for 20, but has contained 25, and might, without crowding, hold 38 patients. The rooms are small, but lofty, and well ventilated. The situation and structure of the building, indeed, are probably inferior to none. It met with the entire approbation of my friend Dr Jackson, whose great experience of military hospitals is so well known. In its internal arrangements. I have followed as much as I could with civilians, the plan of regimental hospitals. The graduated scale of diet has been adopted, and has almost entirely prevented relapses. We admit, upon the recommendation of the Faculty, patients in every stage of fever.

The parish officers pay a guinea a-week for all paupers while there; and those who send their servants, or come themselves, pay the same. The medicines are prepared at the dispensary gratuitously. The apothecary of the dispensary visits the hospital morning and evening, and they are prescribed for by the physician, who registers their cases. The physicians of the dispensary, five in number, are in rotation annually its attendants. The patients admitted between 4th September 1818 and 4th March 1819, during which period they have been under my care, have amounted to 145, of whom 117 have been cured, and 18 remain in hospital, likely to recover. This makes the mortality as one in fourteen and a half. There were in hospital, when it fell to my charge, fifteen, of whom two died. The number, then, that has passed through the hospital during the above period, is 160, of whom 130 have been cured, 18 remain, and 12 have died, making the whole mortality as one in thirteen and one-third. Of the whole hardly one has died who had been early admitted. Most had been either long neglected, or under medical treatment in unfavourable circumstances. Some were old, and others were probably feverish only as symptomatic of the last stage of previous disorder.

Should I have leisure, the particulars may hereafter be more minutely detailed.

Newcastle-upon-Tyne, 10th March 1819.

Observations on Fever and Vaccination, extracted from the Reports of the Carlisle Dispensary for 1817 *and* 1818. *Communicated by Thomas Barnes, M.D.*

1817.

At no time is the necessity or utility of such an institution more obvious than during the prevalence of an epidemic disease. Several epidemics have prevailed in Carlisle since the last report in 1816, as appears from the records of the dispensary. During the spring of 1816, a few cases of continued fever occurred, but it appears to have been of a very mild kind. In the summer of the same year, hooping-cough prevailed with great severity, and to a considerable extent: it disappeared in autumn. In November, measles began to make their appearance, and continued prevalent during the whole winter, but only to a moderate extent. In the spring of 1817, they became more frequent and fatal: they began to decline in summer, and disappeared in autumn. A species of continued fever, which has commonly gone under the name of typhus, and lately excited considerable alarm, commenced in July last, since which time it has continued to spread through different parts of the city and suburbs. This fever has prevailed more particularly among the lower classes, from their want of cleanliness, their scanty allowance of bad food, and their breathing impure air, all of which, by weakening the constitution, render it more susceptible of contagious diseases.

There can be no doubt as to the contagious nature of this epidemic. We have in general traced the exposure of patients to contagion, and the disease has prevailed only in particular districts. Some of the most filthy and confined situations in the town are still exempt from it.

Fumigations of oxymuriatic and nitrous acid gases were frequently employed, but as cleanliness and ventilation could not be obtained, little benefit was derived from them. When one member of a family caught the fever, it generally affected the whole. The mortality has not been great, considering the many disadvantages under which most of our patients laboured. It was not uncommon to see three affected with the fever in one bed; four, six, and even eight, in one small, ill-ventilated room. The average number of deaths has been about one in ten.

Cases of Fever attended from the Dispensary.

		Admitted.		Died.	Cured.
1817.	July	6	of which	1	5
	August	12		2	10
	September	14		3	11
	October	51		4	47
	November	38		2	36
	December	29		2	27
1818.	January	24		4	12
Total number to 3d February		174		18	148

Remaining on the books and under cure 8

1819.

We regret that it is our painful duty to report again the unhappy and fatal consequences of the epidemic fever, of which we gave a short account in the preceding Report; it has been very prevalent during the last year. We shall mention the cases of this fever in the order of their admission.

Cases of Typhus Fever attended from the Dispensary.

		Admitted.	Died.	Cured.
1818.	February	19 of which	0	19
	March	45	1	44
	April	53	8	45
	May	40	5	35
	June	36	6	30
	July	25	2	23
	August	8	1	7
	September	9	3	6
	October	11	0	11
	November	11	0	11
	December	15	1	14
1819.	January	11	1	6
Total number to 2d February,		283	28	251
Admitted in 1817,		174	18	156
Total since the commencement of the epidemic		457	46	407
Remaining on the books, and under cure,			4	

Forty-six cases of natural small-pox have occurred in the practice of the Dispensary during the last year, of which seven died. Eight had been previously inoculated for cow-pox, and one for small-pox. All the eight cases of small-pox, after vaccination, were exceeding mild and of short duration; the pustules were few in number, and generally maturated about the 5th or 7th day; some of them did not maturate, but decayed in three or four days. Four of them were vaccinated at the Dispensary; two five days, one four days, and one only three days, before small-pox made their appearance. The others had been vaccinated by regular medical practitioners, and had distinct marks on their arms. In the case of small-pox after inoculated small-pox, the pustules were numerous and confluent; there were considerable swelling and tumefaction of the face, and great salivation. The patient, a female, had a difficult recovery; inoculation had been performed more than twenty years ago, but there were two large distinct scars on her right arm. A copious eruption having made its appearance at our first visit, marks were not observed on other parts of the body. Where all the children of a family had been vaccinated except one or two, we have repeatedly seen the vaccinated escape, and the others affected with small-pox; and where none of the children of a family had been vaccinated, and one of them caught the small-pox, by immediately vaccinating all the others they have escaped the disease. From this statement, it appears, that neither inoculation for small-pox nor vaccination do infallibly prevent a subsequent attack of small-pox; and that cases of small-pox, after vaccination, are modified and unattended with danger, but that they are of more frequent occurrence than has been supposed. About three times the number mentioned have occurred in Carlisle, within the last year; which, however, are very few compared to the number vaccinated: 416 have been vaccinated at the Dispensary only, during the last year—1644 within the last eight years. If vaccination, therefore, be not a preventive of small-pox in every instance, it is at present, in by far the greatest number; and where it fails in being a preventive, it deprives that loathsome and fatal disease of its malignancy and danger.

No. LX. will be published on the 1*st July* 1819.

THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL.

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Communications have been received from Dr MARSHALL HALL, Messrs JOHN FOSTER, Esq., CHARLES RITCHIE, JAMES KENNEDY, W. M. ROSS, and J. B.

We have received for review the following publications:

History and Description of an Epidemic Fever, commonly called Spotted Fever, which prevailed at Gardiner in the spring of 1814. By E. Hale junior, M. D. M. M. S. S. 8vo, Boston, 1818, pp. 246 and xi.

Practical Observations on the Treatment, Pathology, and Prevention of Typhus Fever. By Edward Percival, M. B. M. R. I. A. 8vo, London, 1819, pp. 156 and ix.

A Practical Treatise on the Efficacy of Bloodletting in the Epidemic Fever of Edinburgh, illustrated by numerous cases and tables, extracted from the Journals of the Queensberry-House Fever Hospital. By Benjamin Welsh, M. D. Superintendent of that Institution. 8vo, Edinburgh, 1819, pp. 150 and xiii.

Observations on the Prevention and Treatment of the Epidemic Fever at present prevailing in this metropolis, and most parts of the United Kingdom; to which are added, Remarks on some of the Opinions of Dr Bateman and Others on the same subject. By Henry Clutterbuck, M. D. one of the Physicians to the General Dispensary. 8vo, London, 1819, pp. 299.

The Hospital Pupil's Guide, being Oracular Communications, addressed to the Students of the Medical Profession, including plain and useful directions relative to the best mode of attending the various branches of medical study; to which is added, an Account of the days and hours of attendance of the Physicians and Surgeons of St Thomas's and Guy's Hospitals. By Æsculapius. Second edition corrected, 18mo, London, 1818, pp. 96.

Observations on Hemeralopia, or Nocturnal Blindness, with Cases and Practical Illustrations. By Andrew Simpson, Surgeon. 8vo, Glasgow, 1819, pp. 132.

Medical Topography of Upper Canada. By John Douglas, Assist. Surg. 8th Regiment. 8vo, London, 1819, pp. 126.

Conversations on Botany, with Plates. 12mo, London, 1818.

An Enquiry illustrating the Nature of Tuberculated Accretions of Serous Membranes, and the Origin of Tubercles and Tumours in different textures of the body. By John Baron, M. D. Physician to the General Infirmary at Gloucester. 8vo, London, 1819, pp. 307. With five engravings.

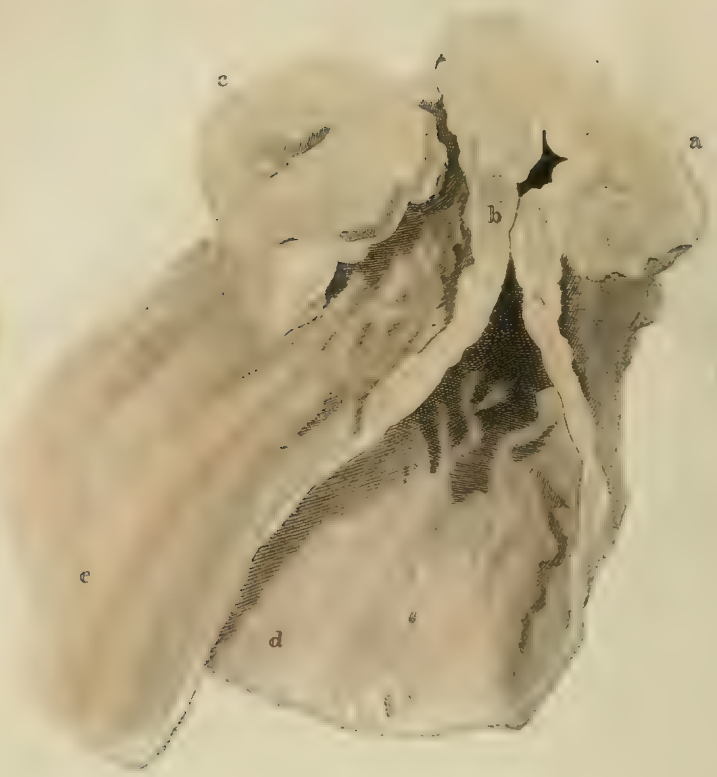
Observations on the Prevalence of Fever in various parts of the United Kingdom, and on the eminent utility of Houses of Recovery; exhibiting the great advantages that would result from such an institution for the reception of the Sick Poor of Bristol and Clifton. By D. J. H. Dickson, M. D. F. R. S. E. 8vo, Bristol, 1819, pp. 34.

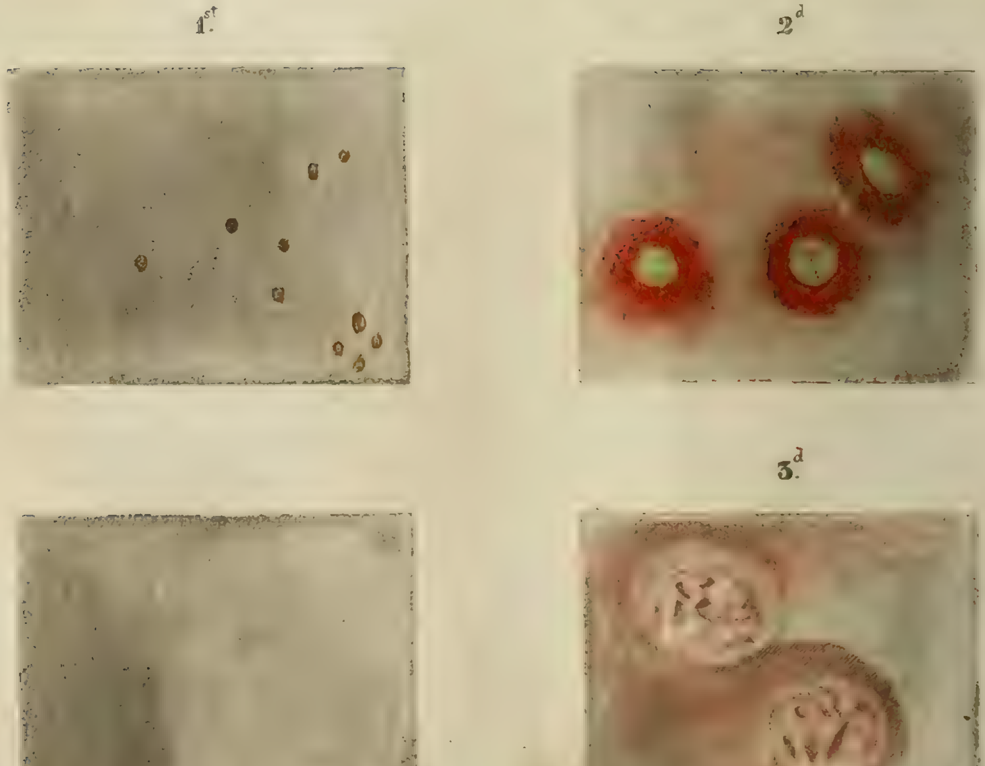
Remarks on the Treatment of some of the most prevalent varieties of Inflammation of the Eye, with Cases. By Thomas Whately, M. R. C. Surgeon, London. 8vo, London, 1819, pp. 132 and vi.

Observations on the Condition of the Middle and Lower Classes in the North of Ireland, as it tends to promote the diffusion of Contagious Fever; with the History and Treatment of the late Epidemic Disorder, as it prevailed in an extensive district of that country, and a detail of the measures adopted to arrest its progress. By Francis Rogan, M. D. 8vo, London, 1819, pp. 159 and xx.

The Modern Practice of Physic, exhibiting the Character, Causes, Symptoms, Prognostics, Morbid Appearances, and Improved Method of Treating, the Diseases of all Climates. By Robert Thomas, M. D. The fourth American from the fifth London edition, revised and considerably enlarged by the Author; with an Appendix. By David Hosack, M. D. F. R. S. Professor of the Theory and Practice of Physic, &c. New York. 8vo. New York, 1817, pp. 915 and xvi.

Lectures on Physiology, Zoology, and the Natural History of Man, delivered at the Royal College of Surgeons. By W. Lawrence, F. R. S. and A. With twelve Engravings. 8vo, London, 1819, pp. 579 and xxiii.





Stages of the eruption in yaws J. Thomson. M.D.

THE
EDINBURGH
MEDICAL AND SURGICAL JOURNAL.
1. JULY 1819.

PART I.

ORIGINAL COMMUNICATIONS.

I.

Observations and Experiments on the Nature of the Morbid Poison called Yaws, with coloured Engravings of the Eruption.

By James Thomson, M. D., &c.

The general appearance of the eruption in this disease has been accurately described by every writer on the subject, but many particulars necessary to an accurate knowledge of the laws that it follows have been entirely omitted, or rather never ascertained. Many British practitioners have exclaimed against this neglect, and the more so, as the disease is of so common occurrence in many of the West India islands, that it comes within every one's observation. To a person who has resided any time in the West Indies, the cause of such neglect is easily explained. There is a sort of actual horror on the countenance of most practitioners at the sight of a patient covered with the eruption. He views him afar off, and his opinion is given in the most hurried manner. The dreadful stories of white people having been infected, the shame attached to it, and, more than all, the fear of his prospects in life being blasted, are sufficient to counteract every desire of advancing his knowledge of the disease. The consequence of this is, that the yaw patients are banished to some remote corner of the estate, where they are put under the care of some old woman, who keeps the sores clean, and conducts them through the disease. If, from neglect, symptoms called the dregs of the yaws should appear, or if any hereditary tendency should have been called into action, the practitioner may again see the patient, but he has only one mode of practice, an alterative course of mercury, and *that* the patient *must* undergo. If the symptoms should cease, in a short time they again return,—a second course must be submitted to, for the first was not sufficient. I am ashamed to mention the number of such courses I have seen a poor creature go through, and I hardly would get credit if I related what has been told me by old practitioners. Such symptoms, I shall afterwards show, are, in general, incurable; all we can promise is, to mitigate them with a generous diet, and the use of the woods employed by the natives. One would have thought that their success might have taught medical men to follow their plan; but no, there is a charm in an alterative course that has sent thousands to their graves. Happily for mankind, the charm is fast losing its power, and in a few years practitioners will ridicule our notions of its virtues. A perusal of the excellent production of Dr Adams on morbid poisons first induced me to turn my attention to this subject, and I trust he will find some of his queries answered in this paper; and nothing could give me greater pleasure than to satisfy the doubts of a physician so eminently qualified to extend our knowledge by his accurate mode of observation.

This disease is constantly preceded by symptoms of languor and pain, amounting in most cases to fever, which is more severe in children. Before the eruption appears, I have frequently seen the whole surface covered with a white scurf. It looks as if the skin was dusted with flour. It remains only a few days, and then small pimples may be seen on the forehead, and other parts of the body. These continue to increase for six or ten days, when they get a crust on the top, and ill formed pus can be detected. The pustules go on increasing in size, covered with loose irregular crusts; many of them acquire the size of a shilling. If you remove this crust, you find a foul sloughy ulcer beneath. I have endeavoured to ascertain the exact period when you can find the fungus to shoot up, but it is irregular. I have found it so early as one month from the eruption, and so late as three. We cannot, therefore, take this as the criterion of the second stage of the disease. It seemed to depend much on the constitution of the patient, and appeared sooner on those that were well fed. A second attack of fever will occasionally bring out a new eruption. These proceed in the same way, so that we have sores in all states on the body at the same time. When the disease attacks the throat, it is extremely distressing; the ulcers resemble a piece of *toasted cheese;* they never have any fungus; and before the susceptibility is destroyed, the patient is deprived of the greater part of the palate.

The yaw shows a difference according to the subject. In healthy patients it is red, like a piece of flesh; when weakly or diseased, it is white like a piece of cauliflower, and bleeds on the slightest touch, and is by no means so elevated. In this state it remains for some time, till the fungus gradually contracts, diminishes in height, and is finally covered with skin. The yaw leaves no mark in general, except in those places where the inflammation has run very high; the scar in that case resembles the cow-pox scar, only it is broader, and more superficial.

The duration of the disease is various. Some will get rid of it in six months, others not till twelve; the average may be stated at eight. The violence of the eruption and fever does not seem to depend, as in other exanthemata, on the peculiar susceptibility of the patient, but merely on his bodily strength at the commencement of the disease; for in weakly ill fed children it is uniformly more severe. The hairs are represented by many authors to be turned white. This I am confident is erroneous, and proceeds only from want of cleanliness. In all cases where proper ablution is used, no such thing can be observed. Where the hair was destroyed, it seldom ever returned, and then it was of the natural colour. The disease is classed among the Tuberculæ in the arrangement of Willan. What right it has to be placed there I cannot imagine, for surely a tubercle and a fungus are quite distinct, and the fungus comes on at a late period of the disease. The eruption is at first papular, then pustular, and after that rises the yaw, but this is not constant; the ulcer may heal without any rising. If, therefore, we must have it to conform to an artificial arrangement, it must be classed among the Papulæ, then transferred to the Pustulæ, and afterwards carried forward to the Tuberculæ; but if no fungus shoots up, it must remain as a pustular disease; or, if we will be still more scientifically minute, those sores that have a fungus must be called tubercular, and those that have not pustular, even though they are within a few inches of each other. Nosology has already led to more absurd conclusions, and it will still continue to involve those who patronize it in more ridiculous difficulties.

The first question I endeavoured to ascertain was, the *latent period.* Chance first informed me of this. A number of healthy negroes were removed from a mountainous situation to a sugar estate. The children were mixed with those already on the property, and had their meals together. Seven weeks after their intercourse, three were seized with fever and pains; the eruption appeared all over the body ; the rest, at the end of ten weeks, showed symptoms of the disease, and in eight months they had all recovered. Not satisfied with this accidental experiment, I was allowed to inoculate a child with matter. I ordered it to be taken from ulcers where the scab had been removed, and inserted in five different places. Three of the punctures healed, the other two did not heal for three weeks ; they resembled a scratch that proves irritable. From that time, small sores appeared, which increased until they became foul, sloughy, ragged ulcers. Seven weeks from the time of insertion, pimples appeared on the forehead, and over the body, the fungus shot up, and the patient had a copious eruption, and did not get clear of the disease before nine months; the sores had no particular appearance. I may remark, that the two ulcers on the arm never showed any fungus, and left deep scars.

The small-pox having broke out in a mountainous situation, recourse was had to inoculation. Matter was taken from an apparently healthy negro girl, who had already been inoculated for the small-pox. This matter was inserted into the arm of only one subject. When the negro girl knew this, she came in great anxiety and confessed she had the yaws. It could not be believed, for her skin was shining, and quite healthy, but on lifting up the arm, three sores were found in the axilla. The patient that had been inoculated had the small-pox mildly. At the end of two months the yaws appeared, and were neither milder nor shorter in their course than usual. The puncture made where the small-pox was inserted healed up in the proper time, and the secondary pustules were all dried up long before yaws appeared. This case might lead us to conclude, that inoculating with the circulating fluid of a yaws patient will produce the disease; but I have since found, that it is not the case, and that there must have been some undiscovered cause of fallacy in the example of the negro girl. I ordered four children to be inoculated, each in five places, with blood taken from a subject covered with yaws. The punctures healed immediately. In this fact, the yaws follows the course of the facts known with regard to other morbid poisons. The case of yaws mentioned by Dr Adams as occurring in a Danish nobleman who had been *ten* months absent from the West Indies, is at variance with the facts just stated. I will not venture to reconcile them. I may merely state, that several old practitioners, to whom I have mentioned the fact, were of opinion, that the latent period could never have been so long. These cases also go to prove that inoculation does not render the course of disease either shorter or milder, and many other considerations confirm me in this opinion.

The patient is liable to other exanthemata during the yaws. I have known them have small-pox and chicken-pox. I ordered several yaws children to be vaccinated. The cow-pox went regularly through its different stages; there was no observable difference in the time of healing in the sores. The matter was not taken in these cases. I did not feel myself warranted in doing so.

Matter was taken from an active yaws sore, and inserted into the axilla of a rabbit and a dog. No effect followed. It was repeated twice, but the punctures always healed. Matter was made up into a bolus, and given to a dog and a fowl, with no effect. There is an eruptive disease in cattle that the negroes call the yaws, but there is no analogy between them.

This disease can only be communicated once; the susceptibility is destroyed; but there frequently happens very unpleasant symptoms to a person who has undergone the eruption. For example, a mother who gives suck to a child with yaws, has the nipple excoriated from the acrid matter around the mouth; the irritation spreads, and a foul ulcer consumes the breast, and produces death by spreading to the axilla; the child also perishes, unless supported by the greatest care, for it is abandoned by every person.

There is a singular fact with regard to the history of the yaws not generally known, but not the less certain. If a person with a large ulcer contract the yaws, he may pass through the disease without any eruption. At a certain time, the sore begins to acquire a new granulated appearance, and becomes elevated, the edges of the ulcer become characteristic, and a constant discharge is kept up. If this sore is attempted to be healed, and we succeed, an eruption of yaws follows in the regular manner. Otherwise, it goes on, and generally remains as an ulcer for life. The practice always to be pursued is, to endeavour to heal the sore the moment we suspect any infection.

I have endeavoured, by every means, to ascertain whether the disease can be communicated by the mother to the child *in utero*, but without obtaining any satisfactory information. Many old negro women have told me, that a child born under these circumstances never has the yaws, but no reliance can be placed on their testimony. Equally suspicious is any information I have on the question, whether the air can propagate the disease. The subject is so open to fallacy, that I would place reliance on no conclusion. To determine at what time the eruption becomes infectious, and when it ceases to be so, would require more facts than what I am yet in possession of.

The practice now generally adopted in this island is to leave the disease to the efforts of nature, to feed the subject with good diet, and do light work. In weakly children, towards the conclusion of the disease, the decoction of woods, flowers of sulphur, or antimonial preparations, are of considerable service.

Any proprietor of negroes is well aware of the loss he sustains from the yaws, not so much from the actual disease, as from the fatal symptoms that too often follow. The finest looking slave will, in consequence of such effects, in a few months become a burden to himself and his master. The yaws have their bitter dregs (as they are vulgarly called) as well as other diseases.

If the patient has been ill fed, or caught cold, and the eruption repelled, he generally dies dropsical. When the disease has attacked the throat, the soft parts are always lost; repeated inflammations ensue; and, from being considered by the ignorant as venereal, the poor creature, to his present misfortunes, has that of a needless salivation added. Mercury is of trifling service. A regulated diet, and the use of the country remedies, is much more efficacious; but the most unpleasant consequence is, that, if there should be any tendency in the constitution to hereditary disease, it is universally excited into action. The patient, shortly after the eruption ceases, begins to complain of pain and stiffness of all the joints. He loses flesh, the bones swell, the voice becomes guttural, there are patches over all his skin, the feet swell, and repeated attacks cause them to remain so permanently; the bones are swelled and deformed, and, finally, some ulcer breaks out, and the patient is rendered an object for life. These, and many others, are called the effects of ill cured yaws, but I am convinced, with little foundation. Symptoms of a similar nature begin to show themselves in many negro children at or before the age of puberty; previously, we could not wish to see finer livelier creatures. They begin to pine away, worms are suspected, and remedies are administered without relief. A sore at last may break out, and the symptoms may here remain stationary, but more generally the disease goes on, and the patient becomes disgusting to every one. He lingers a few years in some lonely spot. By most people in this country these symptoms are called venereal, arising from *rotten* parents, as they are termed. My views of the venereal disease would never allow me to agree to such a doctrine, and, after giving the subject every attention, I am convinced we must look for their origin at a different source. They will be found to be leprous affections in varied forms, arising from the hereditary tendency that is known to exist in such diseases. The confusion that prevails in the history of leprosy shows sufficiently the different forms that climate imposes upon it. The negroes were formerly very subject to it. From better living, and other causes, the tendency has not been roused except in some particular instances; and that severe form of it mentioned by many authors is hardly to be met with.

I am convinced that such cases are not venereal, because such severe cases are not known in this climate. The children at birth are healthy, and remain so many years; the parents are found often to have no disease, but a leprous affection can be traced to some one more remote; the symptoms are not curable by mercury, and it only attacks particular children of a family. The leprous children who derive the disposition from their parents, escape the disease till near puberty. Climate has varied the forms of the leprosy, witness the *coca bea*, or joint-evil. It has many of the characteristics of true leprosy; and has not climate so modified the virulence of the venereal disease, as to enable the constitution to overcome it without the aid of mercury?

I have often been astonished that Mulatto children, generally born of healthy parents, are more subject to these symptoms than black children. I have often suspected, that, in the crossing of the different races of beings, such hereditary tendencies were more liable to be excited. I lately found this conjecture to be correct, from the information of an intelligent gentleman who resided many years in a Spanish colony where leprous affections were very prevalent. He said that the natives have an old idea, that, if a man have connection with a black woman predisposed to leprosy from her family, the offspring will become diseased. This they prove by citing many cases of negro women, whose children, by white men, were diseased, but whose future offspring, by husbands of their own colour, have remained perfectly healthy; and they attribute the exemption of the higher orders in society from these diseases merely to their avoiding the exciting causes.

Whether these remarks on leprosy are correct, others having more extensive opportunities must judge. My chief object in making them is to do away with the abominable practice of the indiscriminate use of mercury. It ultimately ruins the individual, while a more simple treatment would render his life comfortable.

The yaws has been compared with that disease mentioned by Moses in the 13th chapter of Leviticus. If medical men can reconcile them, it is much to their credit. In my humble opinion, that chapter requires a very ingenious commentator to explain it.

I have by me the notes of an old practitioner of this island, who went home to a part of Scotland many years ago, where the *sibbens* was prevalent. His remarks are made without any allusion to theory, and they tend to confirm the identity of yaws and sibbens in many particulars. Observations from a person who had seen both diseases are peculiarly valuable. It would be improper to lengthen this communication with entering on this point at present. I shall take another opportunity of offering you some farther researches connected with this subject.

St Thomas in the Vale, Jamaica,
12*th Sept.* 1818.

II.

Case of Scirrhous Pylorus, and Mortification of the Stomach. With an Engraving. By JAMES PAXTON, Member of the Royal College of Surgeons, London.

JOHN DAVIS, æt. 19, by trade a shoemaker, about three years ago became affected with pain and irritability of the stomach, so that little solid food could be taken without exciting uneasiness and sickness, with violent retching. However, nothing came from the stomach but a viscid mucus, or a watery fluid. From the frequency of these attacks, during the last twelve months, he became thin, weakly, and unable to work at his trade. Often, within this time, the epigastric region acquired an excessive hardness and tumefaction. He had great pain and vomiting, which continued until two or three quarts of chocolate-coloured granulated fluid was ejected. After this, he remained tolerably easy for ten days or a fortnight, when a recurrence of the same symptoms were experienced.

On last Christmas day, after attending divine service, he complained, as usual, of his stomach. In the evening, intense pain was felt in the epigastrium. He was exceedingly sick, and, with a view to excite vomiting, he drank warm water and chamomile tea, and also took two active emetics, but these had no effect in evacuating the stomach. Pain, fulness, and extreme tension, increased, and extended to the whole abdomen.

Injections *per anum* were administered, and anodynes given, without affording the smallest relief. At two o'clock the next morning he expired.

Examination.—The abdomen contained three or four quarts of a dark-coloured fluid. The omentum and intestines were vascular, and, with the peritonæum, exhibited numerous spots of extravasation. Toward the lesser curvature, and near the cardiac orifice, the stomach was mortified, and had given way to a considerable extent, from whence must have issued the fluid contained in the abdomen. The stomach also was unusually large, its coats thick, and the mucous membrane highly injected with blood. Near the pylorus, it was preternaturally hard, and set round with several whitish tumours, two of which were as large as hazel nuts. A section of the pyloric orifice shewed this outlet very thick, unyielding, and contracted to the size of a common quill.

Explanation of the Drawing.

a. A section of the pylorus.
b. The thickened state of the coats of this extremity of the stomach, seen by its being laid open.
c. A tumour.
d. The mucous membrane of the stomach.
e. The peritoneal substance of the stomach.

III.

History of the Epidemic Fever, as it appeared in a Country Parish in the North of Scotland. By William Gourlay, Surgeon, Lentrathen, Forfarshire.

During the early summer months of the current year, a fever of the continued type, bearing the symptoms of the prevailing epidemic, (presently denominated typhus,) began to extend itself to an alarming degree among the different orders of a manufacturing village, about seven miles from this place, with which the inhabitants of this parish had weekly communication on the market days. Its first subjects were the young and plethoric; and, from its running through whole families, it was readily considered as of an infectious nature; yet no case appeared at Lentrathen till the end of August. The first was a stout young man, a miller, who attributed his illness to exposure to cold, when under profuse perspiration; for which, of his own accord, he had taken an emetic, and some purgative medicine, with much relief. Accounting it a sporadic case of simple fever, of a very mild nature, (the heat of the surface being inconsiderable, and no very pressing symptoms present,) I contented myself with the abstraction of a few ounces of blood, which shewed no appearance of existing inflammation, and trusted his cure to repeated doses of calomel and jalap, with the occasional help of diaphoretics. His recovery was protracted, his spirits depressed, and he relapsed after being up and going about. Before this time, however, I was better acquainted with the nature of the disease; and, as he complained of slight headach, with some difficulty in breathing, regardless of the relapse, I abstracted twelve ounces of blood, with the most manifest advantage; for, in four days after being bled, he was able to attend his employment, even to carry a boll of barley to his mill, and now acknowledges himself in better health than he has enjoyed for years. On the same day that this miller was attacked with fever, a young boy in the next house was taken ill with headach, pain in the back and bowels, with a slight degree of purging. He quickly recovered, under the use of small doses of calomel, antimonial powder, and opiate confection, given at bed-time in the form of pill, with infusion of senna on the following mornings; the tepid bath occasionally. No other cases occurred till the end of September, when, in the space of a few days, seven young women, residing in opposite parts of the parish, were attacked with symptoms of fever very similar in each. Four were suddenly seized with pain in the bowels, attended by purging; the others were less violent in their nature. The abdominal complaint soon went off, and the headach and pain in the loins instantly supervened.

From the suddenness of the attack, and the previous healthy and plethoric state of my patients, it immediately occurred to me that the fever I had to contend with was of an inflammatory nature, and my resolutions were formed accordingly. Since that time to the present, I have seen upwards of forty cases of the fever in this parish, all of whom have recovered, some very quickly, others more tardily, which last I can trace to visceral disease, of long standing.

The contagious or non-contagious nature of the fever, as it appeared in this neighbourhood, would easily admit of dispute. The two first cases may be readily accounted sporadic, yet it did not leave the small hamlet where it first appeared, till fourteen persons were affected by it. There are instances of its attacking only one in large families of young people, and there are even instances of young women sleeping with the sick (against my positive injunctions) during their illness and convalescence, and continuing in perfect health; and it may not be unworthy of observation, that during the month of October, when the disease was most common at Lentrathen, the immediately adjoining parishes were perfectly free from it; thus verifying your remark, "that fever often seems to arise independently of all communication with the diseased, and that the most intimate connection is often unproductive of any bad consequence." (Edin. Med and Surg. Jour. No. 56. p. 537.) There are a few families in which four and five of their members were attacked in succession, but I met with none where all suffered by it. Young, healthy, and unmarried females seemed most obnoxious to its influence, and girls in preference to boys. The married and aged seemed to partake of an immunity; the oldest patient I saw was a woman of thirty-five years.

In the months of September and October, the wind was constantly veering from the S. W. to S., S. E. and E., not twenty-four hours passing without rain, most commonly in the night time. The weather was unseasonably warm, even sometimes sultry, and the showers were much like those which follow thunder storms. If this state of the atmosphere was no exciting cause of fever, it certainly prevented a speedy convalescence and recovery. For the two preceding years, the fuel, which is chiefly peat and turf, was taken home in very bad order, owing to the early and unseasonable rains; and the poor, from want of employment, were no doubt scantily fed. This last observation does not, however, apply to most of the cases I attended, for the servants of the small farmers in this part of the country generally live at their masters' table, or with very little variation in the quality of their food.

I was sometimes led to suppose, that the past unusually warm and dry summer might be a strong exciting cause, operating upon the predisposing causes of the antecedent cold and wet seasons; and accounted for its frequency among females, to their not being exposed to out-door labour so much as the men, till, in the hard working time of the harvest months, during which the fever made its appearance. But this would by no means apply in general. Be the causes what they may, I should have no hesitation in believing that the fever, which, in the course of two months, affected forty persons in this parish, was a branch of the national epidemic, which is continuing to extend its influence to the north of this island; and I am induced to send you this imperfect sketch of its appearance in a country parish, as its progress to fatality seemed to be effectually retarded by the prompt depletion you have often recommended in your excellent periodical publication; and as my mite of additional proof that fevers of British origin admit, nay, even require, depletion, to an extent not thought of some years ago.

The most constant, and the most urgent symptom, was pain in the head, generally about the fontanelle, or in the forehead, between the eyes; seldom in the occiput. This affection of the head was sometimes preceded by giddiness, and dimness of vision. In those most suddenly attacked, it began with instantaneous pain in the belly, in some cases followed by purging. The pain of the abdomen generally went off in the course of ten or fifteen minutes, when the head and loins became affected, and continued so till relieved by bleeding. Prostration of strength was immediate, and very great, some falling down at their work in-doors. In a few, the fever was ushered in with frequent fits of deliquium, and slight convulsions. In one case, this tendency was so great, that the patient could not lift her head from the pillow, without syncope coming on, and she fainted three times before I could open a vein; for I was determined to bleed her in a sitting posture. Immediately on the blood flowing, she grew cheerful, and expressed herself as freed of sickness, and relieved from a great weight which lay upon her heart. She bore the abstraction of sixteen ounces, and then fainted. This I attributed to the loss of blood, and tied up her arm, but bled her to the same extent four days successively. She recovered quickly, but at no period did her blood show the least appearance of inflammation. The eye was suffused in some, the vessels turgid; in most, it was glossy and watering, with a slight degree of intolerance of light. In one case only, there was a little deafness in the beginning of the complaint, but an intolerance of noise in all, with a sensation of beating, rushing, or ringing, between the ears. I did not observe the carotid arteries over-powerful, except in one instance, where the determination to the head was very considerable, more like a case of phrenitis than continued fever; and, had it not occurred at this period, when the fever was so prevalent, I should have perhaps considered it as such. There was always, in some period of the disease, a sense of congestion in the portal circle, which they expressed by saying, they felt, as it were, a great load on their heart, or something holding their breath, and pointed to the epigastric and hypochondriac regions. All had some difficulty of breathing; could not make a deep inspiration without pain, either in the right or left side, most frequently in the left, about the situation of the heart or spleen. The breathing was quick, interrupted by sighing. One only of my patients distinctly pointed out the situation of the liver, as the part particularly painful, (although I had too good reason to believe it was very sensibly affected in all;) he complained of pain in the top of the right shoulder, had no cough, and could not lie on the affected side. None had any cough, but several had a slight degree of sore throat. In a few there was a little irritability of the stomach, rejecting medicine given without opium. There was dryness, and an unpleasant taste in the mouth, but the thirst was never urgent, at least not what might have been expected in a fever requiring full depletion. The tongue was commonly whitish: in some very clean; in others, red at the edges, white in the middle, and foul at the root. A good many had a foul, clammy spit; and, in one case, there was a slight degree of salivation, previously to taking any medicine. In one family, the fever announced itself by pain and swelling of the inguinal glands, pains in the legs, thighs, and shoulders, and, after the head became affected, the muscles of the superior eyelids were not under the control of the will; the eyes were closed, and continued so till after bleeding, upon which they could open them immediately, and vision was distinct; the pupil dilated; nor did I notice any preternatural contraction of the pupil in the worst cases. On the commencement of the fever, the urine was scanty and high coloured, voided with some pain. In its progress, it deposited a copious sediment, particularly before convalescence. The evacuations produced by purgatives were uniformly of a bad appearance, and fetid; greenish and slimy, or clay-coloured and copious. A very small quantity of calomel affected the gums in many, and they always had a speedy recovery. The heat of the surface was but little, and few perspired freely, excepting after syncope from bleeding. I saw only two cases in which the cold affusion would have been justifiable from heat and dryness of the skin. They generally complained of cold, especially in the inferior extremities, as far as the knees, with pain in the toes and in the heels. The face was sometimes flushed, oftener pale. In no case was there delirium and coma, or confusion of intellect; for the large bleedings in the outset of the attack seemed, in most instances, to convert the fever into inflammation of some particular viscus. The pulse was never much accelerated, averaging about 98; seldom above 100. In the commencement of the disease it was weak, but incompressible; in many cases scarcely perceptible at the wrist, but to be readily distinguished at the bend of the arm, beating hard. This apparent weakness of the arterial action in the extremities happened in the most inflammatory cases. Only in one *(male)* patient was it full and strong, and the blood did not show any inflammatory covering, but the patient experienced the greatest relief from the evacuation. In another instance, after three bleedings, the pulse became wiry, and then the blood showed much buff. The appearances of the blood were various. In some it was quite natural, when, from the urgency of the symptoms, and the immediate good effects produced by its abstraction, one might have been led to suppose that it would have been cupped and buffy. In almost all, the first bleeding was of this kind; in the next, the crassamentum large in proportion, and covered with a thin strong membrane of a bluish colour; in the third, the crassamentum small, cupped, and buffy, and thus remained till the inflammation was subdued; corresponding with the remarks made in your Journal by Dr Barlow, in his pathological observations. The serum in many was dark green and transparent; in others milky and turbid, with flakes of coagulable lymph lying on it; in which case the next bleeding generally was buffy.

I could observe no critical days. Those who were most freely bled recovered most quickly. Generally in fourteen days after my first visit, they were able to go about in-doors; but the moist and close state of the atmosphere, I am of opinion, much retarded their convalescence. The male patients recovered more speedily than the females, and required less abstraction of blood to subdue the tendency to inflammatory diathesis; and those under twelve years were seldom confined eight days.

The existing prejudices among the lower classes prove the greatest obstacle to the efficient practice of the country surgeon; and I found it no easy matter to persuade them to the necessity of losing blood for the cure of fever, the old people declaring that they had had many fevers, and in their time no such thing was ever allowed, or thought of; not even (till lately) a drink of cold water, although, to use their own forcible expressions, "they were *toasted sick for six weeks*, and often confined to bed for months." Immediately after the doctor's visit, a consultation of the old women in the neighbourhood is held, each of whom has innumerable nostrums to propose, all equally infallible; and although they may dispute the superiority of their own individual plans, they invariably and unanimously agree in overruling the directions of the medical attendant, more particularly with regard to the use of cooling beverage, abstinence, and the free admission of air. Many, if not all, of these now vulgar prejudices, may be traced to the opinions of the profession; for, to this day, we read of supporting the strength of the patient by wines and nourishing diet, when, at the same time, the medical prescriptions are antiphlogistic, and the disease of an inflammatory diathesis. Among the country people, they act exactly on this plan; the most dainty bits are procured for the sick, because, forsooth, they cannot eat the common fare; and they are desired to force themselves to eat, in order to ward off ensuing weakness. This I found a very serious obstacle to overcome, as I could not make them sensible that the patient's weakness depended on causes quite the reverse of want of food. But finesse is sometimes very necessary, and I luckily hit upon a method to remove most objections, and at the same time save the credit of the old women's professional knowledge, and thereby preserve their good opinion. How to account for so many cases of fever happening in a country parish, in so short a time, they were entirely at a loss; and the want of heat of the surface, with the uniformity of severe headach, inclined them to look upon it as a disease *sui generis*, and nothing less than an uncommon visitation of Providence. I accordingly told some of those who, I knew, would soon spread the report, that the present fever was not of the same nature with those they had formerly witnessed, but was certainly of foreign origin; that it was, in short, a foreign fever that was spreading amongst them. This completely adjusted all differences of opinion, putting to rest all pretensions to skill on their part, and made them fall in more readily with whatever I might judge proper to direct.*

At my first visit, I found it necessary to bleed as a matter of course, and the flow of blood was continued till syncope supervened, which, in most, happened upon losing thirty-two ounces. In some less than sixteen produced fainting, and in a few I abstracted forty, and in one, forty-eight ounces, before tying up the arm. I then directed that no fire should be kept in the room where the patient lay; that the windows should be opened, or the house otherwise well ventilated, according to the state of the weather; and that no food of any description should be offered, till the patient expressed a wish for some, and then, only a little dry toast, or oaten cake with tea, should be given; to be plentifully supplied with cold spring water, or boiled water, when cold, and all visitants most certainly excluded, for the benefit of both parties. The first bleeding often caused vomiting, which was always assisted with drinks of tepid water, or infusion of chamomile flowers. To some I ordered an emetic of tartrate of antimony, but to those only who, with much sickness, had a desire to throw up; for I thought the straining caused by the operation of an emetic, in the early stages of the complaint, increased the determination to the head, which, in all cases, was obviously very great. A dose of calomel and antimonial powder, according to the nature of the case, was administered at bedtime, followed next morning with such a proportion of jalap, or infusion of senna, as would procure free evacuations from the bowels. The aqua acetatis ammoniæ was given in the early stages, in the proportion of a tea-spoonful every three hours, with much advantage. The bleeding was repeated, without regard to the duration of the disease, according as the symptoms of inflammation or congestion seemed to demand it, and varied in quantity to the relief afforded. I seldom tied up the arm till the patients expressed themselves free of headach and difficulty of breathing, and could take a deep inspiration without pain. To most the abstraction of blood operated like a charm. So soon as the vein was opened, the headach gradually went off; some expressing this sense by tracing its removal down the course of the frontal sinus to the nose; others as if they felt something creeping down the back; and one patient declared she felt it go gradually down from the occiput, in the direction of the spine, to her thighs and legs, and was finally expelled by going out at the toes. She had no more complaint. They generally bore the after-bleedings better than the first; but I often bled from a large orifice, in order to produce deliquium, and consequent reaction. So great was the benefit they experienced from this evacuation, that at every visit, if they had the least headach, or oppression in breathing, they requested me to take away more blood, if I should think fit, and I always found it produce the same good effect. To remove the pain of the back was more difficult; two or three bleedings seldom made any impression on it; and I was often at a loss to account for its obstinacy in resisting large depletion, which produced such marked advantage on the other symptoms. I am, however, much inclined to believe, that the pain of the back, so constantly attending febrile disease, is a good deal owing to portal congestion, as, in some cases, severe pain in the loins, accompanied by a very distressing sense of fulness and weight in the epigastric and hypochondriac regions, was completely removed by the sudden abstraction of thirty-two ounces of blood. During the exhibition of purgatives, the aq. acet. ammon. and the use of the lancet, I always ordered the constant application of cloths, dipped in cold vinegar and water, to the forehead and temples, and likewise blistered the nape of the neck with great auxiliary benefit. The feet were often bathed in lukewarm water, or wrapped round with flannel wrung dry out of warm water, and the patients always expressed relief from that means, especially in those cases in which the feet and legs were inclined to be preternaturally cold. From those under twelve years of age I did not find it necessary to abstract blood. The tepid bath at bed-time, repeated doses of calomel and antimonial powder, an emetic at the beginning of the disease, and cold applications to the head, together with the aq. ammon. acetatae, were quite sufficient to remove the complaint in a few days. In children, there was often a considerable purging of greenish, slimy, and fetid matters, which I found necessary merely to moderate, but not to check.

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* *To the Editor*.—This report was not promulgated from sinister or unworthy motives, but merely to gain the entire management of my patients, for my practice was entirely gratuitous.—W. G.

From observing the good effects of the digitalis purpurea in incipient inflammation, I was induced to try its effects in some cases of this fever; viz. in those patients where, after large bleedings, there remained slight wandering pains in the head and breast, with difficulty in breathing and compressible pulse; and I am of opinion that it was certainly of use. However, as I could not trust that medicine in the hands of most of my patients, my experience is too limited to offer any thing decided in its favour, but I consider it deserving a better trial, not, however, in prejudice to the abstraction of blood. I am likewise led to suppose, from the inflammatory tendency exhibited in the symptoms of this fever,—the suffused eye, intolerance of light and noise, excruciating headach, oppression in breathing, &c. &c. that a full bleeding or two from the temporal artery, as practised by Dr Burnett in the Mediterranean fever, would have possessed very great power in checking its progress, perhaps with less loss of blood; but, from circumstances which it would be needless to mention, and from the uniformity of success by venesection, I did not adopt that plan, which, however, might have been preferable.

From premature exposure to cold, there were a few relapses, but these were easily removed by the abstraction of blood, or an emetic followed by some purgative medicine. The abstraction of a few ounces of blood, after the patients were up and going about, I always found productive of the most salutary effect in forwarding a speedy convalescence, in those especially who had night sweats about the head and breast. This I would wish to account for in the change of action produced in the constitution. I never found it necessary to give bark, or any medicine whatever, in convalescence, excepting a dose of compound powder of jalap from time to time till perfect recovery.

It may no doubt be observed that the fever, as it appeared in the parish of Lentrathen, an account of which I have attempted to detail, might have been as effectually checked through the judicious administration of purgative medicines. The uniformly unnatural appearance of the feces sufficiently indicated the disordered state of the intestinal secretions, and pointed out the necessity of having recourse to these means; they were therefore never omitted. But it was not only the removal of the present complaint, but the prevention of disease in a chronic form, that I looked for in full and early depletion. A surgeon in the navy, through the medium of your Journal, remarks that phthisis is not now so common in our fleets as formerly; and this he attributes much to the more free use of the lancet in disease of an inflammatory diathesis. And Dr Barlow, in his excellent pathological observations, writes thus: "We find the effect of acute diseases in relieving the system from nervous affections of long standing, frequently recorded and generally allowed. The explanation has been, that the elements of disease, so long lurking in the habit, were by febrile action elaborated and expelled, and I believe it; but I cannot agree that these elements were of any specific nature, or other than the accumulations of redundant nutriment, and of excrementitious matter undischarged. Partly by the increased actions of the system, which constitute a state of fever, but principally by the evacuations employed to relieve this, these excesses are removed,--the system is relieved from an oppressive load,—its functions are again actively and healthfully performed, and the whole constitution is invigorated and improved." (Edin. Med. and Surg. Jour. Vol. X. p. 366.) But from observing the very sudden good effect arising from the abstraction of a very small quantity of blood, in diseases commonly called nervous, and more particularly on remarking the addition of strength gained by convalescents from this fever immediately after venesection, I am inclined to look upon the time as being too short, in this instance, for allowing the discharge of accumulations of redundant nutriment and excrementitious matters, as the manner of explaining its operation on the system, and rather to suppose that it is from allowing or assisting the change from diseased to a new and healthy action in the constitution, that bleeding proves so necessary and beneficial. In testimony of which, I beg leave to add the following quotation from Mr Robertson on Pneumonia, (Edin. Med. and Surg. Journal, Vol. X. p. 195,) which I am sorry I had not seen before my "Hints on Constitutional Disease" were sent off for your consideration: "Venesection thus practised" (i. e. largely in the commencement of acute disease) "produces a very powerful impression, no doubt, on the nervous as well as on the sanguiferous system; and if syncope is superinduced by it, the morbid agitation of the circulation is at least interrupted, and nature has an opportunity, as it were, of beginning anew, and setting on foot a healthy action."

Some years ago, it might have been speciously argued, that the treatment of fever in the robust and plethoric constitutions of the Scottish peasantry could be no rule of practice when prescribing for the same disease as it appears in the habits of sedentary mechanics. It is, however, now sufficiently proved, that the inhabitants of a crowded metropolis, whose systems cannot be supposed to bear a very high degree of excitement with impunity, so far from being debilitated by the loss of blood, acquire additional strength under its abstraction, and demand the free and frequent use of the lancet; Dr Bateman having most judiciously remarked, that those whose constitutions can support a certain degree of inflammation, can as certainly allow, with advantage, a proportion of depletion adequate for its removal.

TABLE shewing the prevalence of fever in the months of August, September, October, and November, 1818.

Date.	No. of Adult Males.	No. of Females.	No. of Boys.	No. of Girls.	
Aug. 27	1	0	1	0	Only one case from 9th to 21st November, and none since. Weather continues the same.
Sept. 24 to Oct. 31.	3	22	2	6	
Nov. 1 to 21.	3	4	1	1	
	7	26	4	7	

Before entering on the detail of a few cases, I may just observe, that, during the month of October, several young men called upon me, complaining of slight headachs, giddiness, and sickness at morning time; pain and stiffness of the joints, listlessness and languor, which generally left them after breakfast, and did not prevent them from following their usual occupations. As these were very suspicious symptoms, especially about this period, I always bled them ad deliquium animi, and ordered them to take, for several days successively, a proportion of some purgative medicine, which generally removed their complaints. There were likewise a few cases of fever in the parish which I did not see, but I do not suppose they were above five or six. These two classes are not comprehended in the foregoing Table.

CASES.

24th September 1818.—Visited MARY ALEXANDER, ætat. 18, of a full habit of body. Catamenia regular. Was last night seized with violent pain in the abdomen, immediately followed by purging, which has now left her. Complains of severe headach, great weakness, pain of the back and limbs, with sickness, and some desire to throw up. Tongue dry and white; little heat of surface. Pulse 98, and small. Took 14 oz. of blood from the arm at a large orifice, which caused syncope, followed by vomiting.

Habt. hora somni Calomel gr. iv. Pulv. antimon. gr. iij.

25th.—Vomited much last night after I left her, which was assisted by copious draughts of tepid water. Passed a tolerably good night; perspiring freely for a short time. The calomel has not produced any evacuation from the bowels. Skin soft and moist. Tongue clean. Not much headach, or general uneasiness.

Capt. statim Magnes. sulph. ℥iss.

26th.—Got up yesterday against orders, feeling almost well. Salts operated fully. Had four grains of calomel and three of antimonial powder at bed-time. Slept well. Bowels free. Some pain in the back and arms. Slight headach. To take a tea spoonful of the aq. ammon. acet. every three hours. At 3 o'clock, P. M. complained of pain in the right side; increased on attempting to cough, or on taking a full inspiration. Pulse small and sharp. Took from the arm, by a large orifice, 24 oz. of blood, which completely removed the affection of the right side. To take an ounce of Epsom salts early to-morrow morning.

27th.—Had a good night. Salts have not yet operated. Slight headach. No pain in the right side. Skin a little warmer than natural. Pulse regular. To have four grains of calomel, and three of antimonial powder immediately, and the same medicine next morning, if the bowels are not very freely opened.

28th.—Much better. Several evacuations from the bowels. Skin pliable, and covered with gentle perspiration. Headach not quite gone. No recurrence of pain in the side, back, or limbs.

Habt. stat. sulph. magnes. ℥iss.

29th.—Salts operated strongly. Skin cool, but dry. More headach and wandering pains in the limbs. Fixed pain in the back, not very severe. Pulse small, quick, and hard. Blood formerly drawn had no appearance of inflammation. Bled her to 16 oz. upon which she felt completely relieved. This blood shewed some buff, and was cupped at 3 P. M. On account of the appearance of the blood, I now abstracted 10 oz. from the same orifice, which caused fainting; this, however, soon went off. At 6 P. M. is quite cheerful and free of all complaint. Skin cool and natural. Pulse regular and soft. Blood cupped, but not buffy.

1st October.—Has now no fever. Able to sit up and go about the house. Tongue clean and functions regular. Convalescent. N. B. Was bled in convalescence for night sweats about the head and breast.

CASE II.—4th October 1818.—ELIZABETH DUNCAN, ætat. 30, unmarried; catamenia regular, and lives in easy circumstances. Has for some days past experienced pain in the forehead between the eyes. Is sick on rising from bed, which soon after goes off. Has an unpleasant taste in the mouth, and some pain in the breast. Pulse slow, labouring, and rather full. Bled her to 32 oz. from a large orifice, which caused considerable faintness and some vomiting. Suspicions of fever.

6th.—Sent for. The fever is now shewing itself. Great pain in the forehead, and in the breast under the sternum. Breathes quick and laboriously. No thirst, nor great heat of surface. Skin dry. A little sick. Pulse quick, small, and hard. Bled her to 32 oz. when fainting supervened. After recovering she expressed herself free of headach, and pain in the breast, and sickness. Is covered with profuse perspiration. To have infusion of senna.

7th.—Senna opened the bowels freely. Evacuations bad coloured. Slept a little last night. Some headach to-day, but no pain in the breast nor difficulty in breathing. A sense of rushing in the ears. Great pain of small of the back, with a very distressing sense of weight on her heart, (pointing to the epigastric region.) Skin cool. No thirst. Pulse small and incompressible. Blood drawn yesterday is slightly inflammatory. Bled her to 32 oz. which brought on faintness, but no syncope. Surface was covered with a cold and clammy sweat. Ordered some warm gruel, which caused a profuse perspiration. She then grew cheerful, and gratefully expressed herself as being quite well.

Hab. cras mane Calomel gr. v. Pulv. antimon. gr. iv. M.

9th.—Sleeps well. Has little headach, and is otherwise free of pain. No thirst. No heat of surface. Pulse regular, and very compressible. Belly rather slow.

Hab. stat. Calomel gr. vi. Pulv. jalap. comp. gr. xxv. M.

After this report she required no further attendance, taking occasionally a dose of calomel and jalap till health was perfectly restored.

CASE III.—18th October 1818.—BELL NICOLL, ætat. 26. Unmarried. Catamenia regular. Was yesterday much affected with some distressing family affairs. Has been previously in good health, and is of a healthy habit of body. Complains of severe headach, especially about the fontanelle and temples. On being first seized had a considerable degree of pain in the back, which soon went off and has not returned. Skin cool. Pulse low and weak, but sharp and incompressible. Opened a vein, and allowed the blood to flow till she was completely relieved. Headach going away gradually, and continuing longest in the right temple. Bore the abstraction of 40 ounces without fainting. After tying up the arm syncope supervened. Recovered very quickly on lying down.

Cap. hora somni Calomel gr. vi. Pulv. ant. gr. iij. M.

19th.—Did not sleep well. Had no pain, but was very uneasy. Has no complaint. Pulse still low and hard. Blood has a slight bluish membrane covering the crassamentum. Serum turbid. No stool. To take infusion of senna immediately. At 8 o'clock P. M., senna has procured only one evacuation. Headach severe since afternoon. Pain of loins very distressing; some giddiness; tongue clean. Pulse hard. Bled her to 18 oz.

Cap. stat. Cal. gr. iv. Pulv. antimon. gr. iij. M.

20th.—Slept tolerably well. Little relief from the bowels. Headach and pain of back continues. Cold applications to the head since yesterday. Blood taken away last night cupped and buffy. Bled her to 24 oz. which caused some vomiting. To have a dose of salts or infusion of senna. At 7 P. M. has taken both salts and senna without free evacuations. Blood still buffy. Complains of much oppression about the præcordia. Pulse as before. Bled her to 16 oz. To take ten grains of the aloetic pill, and continue the cold applications to the head.

21st.—Took half an ounce of salts this morning, which caused several loose stools during the day, of a natural appearance. At 7 P. M. headach worse; eyes painful, and slightly intolerant of light; pupils not contracted more than usual. Skin pretty cool. No great thirst. Some sore throat. Pulse low and sharp. Bled her to 16 oz. which instantly relieved the head and eyes. The aqua ammon. acet. as usually prescribed. Cold applications to the head, &c.

22d.—Had some sleep in the first part of the night. About 12 o'clock was seized with a violent pain in the left shoulder. It soon went off, and the headach returned with a slight epistaxis from the right nostril. Had a stool in the night, whitish and very loose. The most pain in the right side of the head. Right leg benumbed. Bled her to 24 oz., which caused syncope; upon recovering from which she was covered with a full and gentle perspiration. The pulse is now fuller and soft. No thirst. Cont. aq. ammon. acet. and topical applications to the head. At 6 P. M. headach has just returned, with much pain in the right eye, which is watery. Right leg quite well. Blood last drawn cupped, but not buffy. Pulse full and 100. Bled her to 16 oz. which gave her considerable ease. Bowels very open, but evacuations of a bad colour.

Capt. cras mane Calomel gr. iv. Pulv. jalap. comp. gr. xxv. M.

23d.—Physic is operating. Pain of head and eyes relieved. Blood still cupped. Serum pure. Some pain in the loins.

Cont. cold appl. and aq. a. acet.

27th.—Since last report has been gradually recovering. Sleeps well. No noise or pain in the head. Skin moist. Pulse regular, &c. &c. Is able to sit up in bed, and is convalescing.

N. B. This patient's recovery was remarkably quick after being able to go about.

CASE IV.—21st October 1818. HELEN GRANT, ætat. 28, of a very gross habit of body. Catamenia regular. Got out of bed this morning in good health. About ten o'clock was suddenly seized with acute pain in the abdomen, which soon went off, and headach, with pain in the loins, instantly supervened. Face flushed, skin rather warm, and tongue clean. Pulse hardly perceptible at the wrist, but beats hard at the bend of the arm. Bled her to 40 oz. when syncope came on, with slight convulsions, and vomiting soon followed, from which she experienced relief. Her head is now quite free of pain, but on lying down in bed the pain of belly and back became severe. To have infusion of chamomile flowers to cause vomiting. At seven P. M., headach returning, and abdominal pain less; says she feels it passing gradually upwards on the right side to her head. Blood drawn shows no degree of inflammation present. With some difficulty, on account of the smallness of the veins, and the patient's obesity, I succeeded in taking away 12 oz. of blood, which removed the headach, but the complaint of the abdomen recurred, with some vomiting.

Capiat hora somni Calomel gr. viii. Pulv. antim. gr. vi., et opii puri gr. $\frac{1}{2}$ in form. pilul.

22d.—Physic produced only one stool. Has headach, pain in the back, bowels, and shoulders. Skin cool; no thirst; no sickness. Pulse small and incompressible. Bled her to 16 oz. To take infusion of senna directly, and four grains of calomel, with twenty-five of compound powder of jalap at bed-time.

23d.—Much better this morning. Physic operated well during the night; evacuations clay-coloured. Less pain every where. Skin rather warm. Perspired freely, and got some sleep during the night and this morning. Pulse small and quick, and a little hard. Cool regimen, with a dose of calomel and jalap to-morrow morning.

25th.—Some degree of irritability of stomach. Has rejected, by vomiting, the calomel and jalap powders. Eight grains of calomel, and four of antimonial powder being combined with half a grain of opium, were given last night, and retained. Produced three copious stools, of bad colour, and very fetid. Complains of being cold. Some heat of surface. Had slight shiverings in the morning; does not make much complaint. Pulse quick and sharp; tongue clean and reddish. Some sore throat, and gums swelling. The mercury seems to be taking effect on the constitution.

26th.—Did not sleep well. Mouth and tongue very sore, Throat swelled. Gums spongy. Is somewhat salivated. Has no headach, or pain of loins. Skin moist and cool. No thirst. Pulse quick. Bowels rather bound.

Cap. stat. Pulv. jalap. comp. gr. xxv.

8th Nov.—Since last report, has been much salivated, and unable to give any account of her complaints. She now says, that so soon as her throat and tongue swelled, all the uneasy symptoms left her, and she had no pain any where but in the mouth.

N.B.—Whenever she could swallow any thing, I urged her to take strong infusion of senna, as a means of keeping her bowels open, by which she had passage frequently. She had a speedy recovery, after the swelling of the fauces left her, and I attribute her cure to the constitutional effect of mercury on her system, although the previous bleedings, no doubt, powerfully assisted the operation of the mineral.

It would be very needless, nay, highly improper, to take up more of your time with a detail of cases so much the same, both in symptoms and treatment; and I shall only add, that, from having the opportunity of seeing my patients early in the disease, and receiving their confidence, I was enabled, through the blessing of Providence, to bring them all to a speedy and successful termination.

Lentrathen, Forfarshire,
20*th November* 1818.

IV.

Remarks on the Prevailing Epidemic, and on the reputed Effects of Mercury in Fever. By J. B. Sheppard, Member of the Royal College of Surgeons in London, and Surgeon in the Royal Navy.

The interest excited by the present epidemic diffusion of fever throughout the empire, will be a sufficient apology for the obtrusion of the following remarks, which have arisen out of the treatment of those forms of the disease which have prevailed in this place for several months past. At the same time, I shall take occasion to revert to the question of the mercurial treatment of fever, on which subject I threw out some observations in the 52d Number of the Edinburgh Medical and Surgical Journal. In that communication, my animadversions on the practice were limited to the danger of an exclusive reliance on that remedy in the formidable endemic of the West Indies. I shall now endeavour to shew, that many cogent objections apply to the exhibition of mercury, *as a sialogogue*, in the ordinary contagious fever of this country, even co-operatively with other means; and that, although the danger of trusting to such effects be obviated, by its association with remedies of undisputed efficiency in fever, the advantages apparently derived from the combination are not the less illusory.

The superinduction of salivation, in addition to other means of cure, has, of late, been strenuously recommended in the fevers of this country; and it seems not unreasonable to believe, that mercury may, at no distant period, acquire the ascendancy in their treatment, which it once possessed in the regions of yellow fever. The prediction would seem to be justified by the precedent of the eagerness with which the practice was embraced in the treatment of the western endemic, after the announcement of its alleged superiority over other remedies. If, under the disadvantage of being opposed to a disease of inordinate violence, and of most unparalleled rapidity of career, the evidence of the efficiency of mercury was deemed to be conclusive, and led to general adoption, it requires no great effort of imagination to suppose it invested with equal reputation in the fevers of Britain; the comparative mildness of which, as opposed to tropical fever, (unless under artificial aggravation,) gives comparative facility to the accomplishment of the intended effect.

The high respectability of many of the advocates for mercurialization in our fevers, entitles the proposal to a dispassionate and attentive consideration; and as, under the existing prevalence of fever, suspense on a question of no mean import in its treatment, cannot but be a source of regret, it becomes desirable that attention should be invited to the opportunity afforded by the present epidemic, of bringing the disputed points to an issue, by instituting comparative trials of the efficiency of the mercurial and non-mercurial practices in parallel states of the disease. Having formerly entered into the discussion in reference to tropical fever, it was improbable that I should be indifferent to its prosecution on the present occasion. In stating the results of the instances in which I have again submitted the powers of mercury in fever to the test of experiment, I hope to be allowed credit for having resumed the inquiry, unbiassed by the influence of antecedent opinions. My motive in recurring to the subject is, simply to promote the extension of the investigation, with the view to elicit the truth, and establish, on the faith of others, the degree of merit which the practice may in reality possess.

The first cases of the epidemic in this town were observed about the middle of July last. The disease appears to have originated with, and, until lately, has been limited to the indigent class of inhabitants, residing in crowded, filthy, and ill-ventilated situations. A considerable proportion of the attacks has presented no remarkable deviation from the order of symptoms characterizing the ordinary contagious fever of this country; a disease familiar to common observation, and of which we already possess a superabundance of descriptions. In its progress, however, the epidemic has exhibited some modifications, apparently derived from the influence of season. Before the accession of the winter months, a diarrhœal form of the disease was frequent, and proved extremely untractable. The fecal discharges were profuse in quantity, very offensive, and in appearance resembled yeast. Griping was seldom a cause of complaint; but the evacuations were excited by whatever was taken into the stomach, either of diet or medicine; and in the more violent seizures, the power of retention was lost, even in the first days of the attack. The accession of this form of disease occasionally resembled an attack of cholera; but as the violence of the gastric and intestinal disorder subsided, formal fever was developed, and ran its course, unchanged by the difference in the mode of seizure.

As in this character of attack, the alimentary canal seemed to bear the *onus* of the disease, it might have been inferred, that the excessive alvine discharge would have mitigated the determination to, or congestion in other organs, at least in those not liable to be involved by contiguity. No such salutary effect has, however, been observed; on the contrary, the cerebral affection has been particularly violent and unremitting, where this anomaly has been most conspicuous. Diarrhœa was excessive in three of the four fatal cases which have occurred, and in one of them it continued with unabated frequency until the hour of death, on the 53d day of the attack. Professor Hufeland has recorded the prevalence of a similar form of disease, in an epidemic fever during the first Prussian war in 1806, 1807, in Poland and Prussia, the seat of the war, in which he found wine and opium eminently serviceable.* I have exhibited those remedies in this form of the present epidemic, but without deriving the same advantages from their employment. It is, however, proper to state, that they were given after the failure of other means, more as a matter of experiment, than from any fair indications of the necessity of stimulants.

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* Edinburgh Medical and Surgical Journal, No. 51.

On the approach of the winter season, the diarrhœal was succeeded by a pneumonic form of the epidemic, which has been subsequently its most common modification. The dyspnœa and cough are urgent, and the expectoration is occasionally bloody; but, as under the diarrhœal form, the cerebral and spinal affections are very conspicuous, and with the early prostration of strength, form the principal diagnosis from uncombined pneumonia.

In several of the more protracted cases, towards the end of the fourth week, the disease has simulated the recurrent type. The paroxysms were marked by distinct, and often violent rigors, which were succeeded by intense heat, and increased vascular energy, terminating occasionally in profuse sweating, but more commonly in a gradual subsidence of the exacerbation, without any relaxation of the surface. The paroxysms shewed no obedience to periodicity; in some cases recurring in a few hours, and in others, only after the lapse of as many days. Whether this change of character in the disease was really critical, or otherwise, I do not pretend to determine; but it was uniformly succeeded by a speedy declension of fever.

Inflammation of the parotid and submaxillary glands has also been lately a frequent occurrence, (more especially among children,) in some instances terminating in suppuration. Several individuals of the same family have been usually affected in this manner at the same time; and the disease may probably have depended on a contagious cause. The dissimilarity of the character of the accompanying pyrexia to that of the epidemic precludes, however, their reference to a community of origin.

I may state incidentally, that I have not observed any instance of what has been termed the congestive form of the disease; a state of oppression and congestion without succeeding excitement. Organic congestion is, undoubtedly, a common effect of febrile excitement, and a frequent immediate cause of death. But I have not detected the existence of permanent and unequivocal congestion in fever, except as the sequel of the reactive stage. I do not, however, presume to advance its non-occurrence among the cases which have come within my notice, as an argument against its occasional existence. There is, certainly, no difficulty in admitting such a formidable result from the operation of a highly concentrated contagion; on the contrary, the analogous phenomena of the eastern plague countenance the supposition of its possible existence. The reality of this variety of our contagious fever has, however, been denied by persons of the greatest experience, and accuracy of observation; and while their high respectability secures them from the imputation of having asserted the nonentity of "congestive typhus," unsupported by competent discrimination, and adequate practical knowledge of the disease, the fact of their having questioned the truth of its existence will be, probably, admitted to be strong evidence of its extreme rarity.

I coincide with those who consider the distressed situation of the lower orders to be a principal cause of the prevailing disease. It is not my office or inclination to descant on the privations to which that class of the population of this country has been, for some time past, exposed; those whose avocations introduce them to a familiar acquaintance with their nature and extent, will find no difficulty in recognizing, in those circumstances, a fertile source of fever. It must, however, be conceded, that the evils of poverty operate more especially in creating a predisposition to fever; and it is obvious also, that the condition of the poor is not so essentially changed as to afford, of itself, an adequate explanation of the present extensive diffusion of the disease, beyond that of former periods. The superaddition of some general atmospheric influence must, therefore, be acknowledged; an epidemic constitution, arising at certain periods, and cognizable only by its effects. But, although fever appears to have most frequently originated, and to have been, from time to time, produced *de novo* in the habitations of poverty, there is no doubt that it has been greatly multiplied by the operation of contagion. It has already been stated, that for a considerable time the lower classes alone were the subjects of the epidemic; of late, persons in the enjoyment of every comfort have been almost equally obnoxious to its attacks. In many of these instances, fever has been, unquestionably, self-generated; but the greater number of seizures appears to have been derived from casual communication with the infected sources: In some cases which have come to my knowledge, the contagion has been carried from the town to the villages by servants, who have returned ill to their friends, and have infected whole families. I am aware of the obscurity in which the question of contagion is involved, from the numerous sources of deception to which all reasoning on the operation of a non-tangible subject is exposed; but in reference to the present epidemic, the instances of contagious propagation have been numerous and unequivocal, while the negative evidence afforded by the escape of the attendants and others who have had access to the sick, is too inconsiderable to affect the preponderance of the facts in favour of the opposite opinion.

The treatment has been regulated by the reigning tendency of the epidemic, and the individual complexion of the case. When the curative indications have not been modified by the presence of diarrhœa, or other adventitious symptoms, the lancet has been employed with freedom in the commencement of the attack; and in many instances the abstraction of blood has been repeated during the progress of the disease. A sensible mitigation of the more prominent symptoms, with relief from the general feeling of oppression and anxiety, usually succeeded the evacuation: But even when its advantages were less decisive, depletion was considered to exert an important and salutary effect, in moderating the violence of the excitive stage of fever. It was had recourse to, therefore, whenever the assemblage of symptoms exhibited a degree of violence which denoted the probable superinduction of organic lesion. An emetic was early administered, and was succeeded by an active purgative, repeated at intervals, so as to preserve a free state of the bowels. Cold or tepid ablutions, regulated by the condition of the surface, were seldom omitted, and were found to be equally grateful and beneficial. Blisters were applied, whenever their utility was indicated by the urgency of topical affection; and with antimonials and saline medicines, comprehended the routine of treatment during the presence of fever.

In the milder cases, the abstraction of blood was not judged to be necessary, and the subordinate means adverted to were confided in alone.

In the diarrhœal form of attack, the lancet was also had recourse to in several of the earlier cases, but its effects did not encourage perseverance in the remedy; and one of the cases of that character treated by depletion, belongs to the number of the few which have terminated fatally. Purgatives, and even the mildest laxatives, seemed to be injurious, and to render the subsequent management of the intestinal disorder more difficult than when they were omitted. An emetic has commonly been given in the beginning of the attack, and in some cases it has been succeeded by a laxative. A blister has been generally applied to the epigastrium; and as the disease advanced, absorbents, with or without opium, have been freely given, occasionally with the addition of some Port wine, or brandy and water. Others have been confided throughout to a combination of calomel with opium. But I am free to confess, that in the more severe form of this diarrhœal complication of the epidemic, every attempt to control the violence, or to correct the cause of the intestinal derangement, has been generally fruitless, until, with the declension of fever, that symptom also spontaneously moderated.

In the pneumonic complication, the abstraction of blood has been practised with the greatest freedom; and, indeed, constituted the chief means of reliance. The blood was occasionally, but not uniformly buffy. In one instance, in a robust young man, I repeated the bleeding four times before the pulmonic affection was finally subdued; the last bleeding was on the 17th day of the attack, and was succeeded by perfect recovery. I advert to this case more particularly, because in it, depletion by bleeding was carried to a greater extent, and employed at a later period of the disease, than in any other instance which came under my care; and because it places in a strong light the chimerical nature of the fears so generally entertained respecting the *safety* of that remedy in the contagious fever of this country.

The free admission of pure air into the apartments of the sick has been rigidly enforced, as well for the advantage of the individual labouring under fever, as for the security of the necessary attendants. Petechiæ have not been observed in a single instance; and their absence may, probably, be ascribed to the cool atmosphere in which the sick have been constantly placed.

The duration of the disease has been various: The cure has been rarely abrupt, owing, perhaps, in some degree to the common neglect of application for assistance, until several days of fever had elapsed; but depending more especially on the specific nature of the disease, which, when fully formed, may admit of essential mitigation, but derides every attempt to subdue it *instanter*.—It may be generally disarmed of its danger, but its progress cannot then be arrested, and we are compelled to be content with conducting it to a safe, although, probably, a protracted termination. Fever seldom subsided before the end of the second week; it was more frequently extended to the conclusion of the third or fourth, and in one of the fatal cases, the event was protracted to the 53d day of the attack.

The aggregate result may be considered fortunate. Of 146 cases which have devolved on my charge, four only have terminated fatally.

I have stated that, in several instances, mercury has been superadded to the general means of cure, with the view of obtaining comparative results from the salivatory and non-mercurial practices. Previous to the accession of the pneumonic complication, I selected, indifferently, fourteen cases for that purpose. It would have been easy to have multiplied the number of instances, but when the epidemic had begun to exhibit the pneumonic tendency, I considered the use of mercury as a sialogogue, to be inadmissible; and the results of the experiments already instituted, afforded me sufficient corroboration of what I had so repeatedly witnessed elsewhere,—the facility of exciting the specific operation of mercury in mild attacks, and its absolute inertness in the more concentrated forms of fever, where alone such effects would be desirable. I, therefore, felt myself compelled to abstain from subjecting a greater number to the chance of the inconveniences of a fruitless process, which, from the evidence already obtained, seemed to depend for its whole support on the common delusion of the *post hoc, propter hoc* inference.

Of the fourteen persons to whom calomel was administered as a sialogogue, six had been previously bled, but in six only were any specific effects of the remedy excited, four of whom had been prepared by the lancet. Considerable salivation came on in those instances, after the exhibition of a moderate quantity of calomel; and fever finally ceased. In the other eight cases subjected to mercurial treatment, not the slightest impression was made on the disease, or on the constitution, although the calomel, with or without opium, was steadily persisted in at short intervals, until, as in parallel cases treated without mercury, the patients gradually emerged from a state of fever to that of convalescence. I conclude, therefore, that, in ascribing the cessation of fever in those instances in which salivation ensued, to the change produced in the system by the specific operation of mercury, the reasoning would be inconsequential; and that, to use the words of the poet, "*Decipimur specie recti.*" On the contrary, I conceive the just inference to be, that the preparatory bleeding and other evacuations had effected a mitigation of the fever to a degree not incompatible with the mercurial impression; and that recovery would have been as speedy and as secure, had no mercury, as a sialogogue, been given; because, in the other cases treated with mercury, the disease having been of a more concentrated form, resisting evacuations, that remedy proved to be utterly impotent, although the quantity administered far exceeded that which was sufficient to excite active salivation in those milder cases which were within the control of the lancet and other evacuants.

To the conclusion, that, in fever, the unsusceptibility of mercurial influence depends on the concentration of the disease, it may be objected, perhaps, that individuals are sometimes met with in whom the system resists the mercurial impression, even when no fever is present; and that, in an estimate of the powers of mercury in fever, such inherent unsusceptibility ought not to be disregarded. The fact of unsusceptibility from idiosyncrasy is not contested, but it is presumed that in the adult its occurrence is rare; and the objection is, probably, more than counterbalanced by the existence of an opposite idiosyncrasy, (which is much more frequently observed,) in which the most minute quantity of mercury is sufficient to produce salivation. In children, indeed, the difficulty of exciting salivation, independent of the presence of fever, is far greater than in matured persons. But children are so seldom the subjects of ptyalismal treatment of fever, that any peculiarity of this nature which they may possess, may be properly omitted in reasoning for or against the powers of mercury in fever generally. The immunity of extreme youth is not, however, perfect. Some time since, I visited three children from two to six years of age, who were labouring under a violent degree of salivation, excited by the inunction of some mercurial preparation for the cure of psora. The precise quantity or quality of the combination employed I am unacquainted with, but the ptyalismal effect was very distressing and protracted.

The engagement of the system in diseases of a particular nature, (as the exanthemata,) which have a prescribed course to run, must also be admitted as a cause of resistance to the operation of mercury.

These conditions may be advanced as exceptions to the position, that mercurial unsusceptibility in fever is in the ratio of the concentration of the disease. But I submit with deference, that, however those exceptions may affect the absolute, they leave untouched the general applicability of the rule.

There are other states which are recognised as opposed to mercurial impregnation, as an inflammatory diathesis, and deranged function of the nervous and absorbent systems. These conditions, however, belong essentially to fever, and may, therefore, with propriety be resolved into the general imputed cause of resistance under fever, the degree of concentration.

I may remark, that the mere presence of salivation does not necessarily arrest fever, neither is the coexistence of the two states incompatible in all cases, though generally so, as I lately witnessed in an attack of the epidemic, in which not a particle of mercury was used, but where spontaneous salivation came on in the first week of the illness, and ceased only with the declension of fever.

The result of the cases above enumerated, treated *without* mercury, will be, probably, admitted to be a valid argument against, at least, *indiscriminate* salivation in fever. In order, therefore, to avoid the imputation of pursuing a practice so palpably extravagant, it is incumbent on the eulogists of mercury to express distinctly what are the phenomena in primary fever, which especially demand its supplementary aid, on the presumption of the incompetency of other means. I should anticipate some embarrassment in the attempt to form such pathognomic distinction; the earlier symptoms affording a very uncertain criterion of the subsequent character of the disease. Dr Bateman, in reference to the more severe and complicated form of the epidemic, justly observes, " The greater severity of this form of the fever was not manifest from its early symptoms. During the first week, and often till the eighth, ninth, or tenth day, or even later, it exhibited the ordinary appearances of the more simple fever above described."—(Bateman on Contagious Fever, pp. 52, 53.)

It may appear, perhaps, fastidious, thus to impugn the truth of a doctrine so plausibly constituted, as to hinge on the imposing coincidence of the declension of fever, and the supervention of salivation. But, in conclusion, I must repeat my belief, for the reasons above assigned, that the reputation of mercury in fever has been derived from a fallacious source, that when its uncombined exhibition has been followed by salivation and recovery, the disease has been of a mild grade, which would have yielded to ordinary measures with greater promptitude than to mercury alone, without the inconvenience of the consecutive salivation, and that, when it has been associated with depletion and other evacuants, the extinction of fever has been effected by those remedies, and not by the specific operation of mercury; the system under fever being susceptible of that effect, only when the violence and danger of the disease have been removed by such efficient preparation.

Witney, Oxfordshire, March 21st 1819.

V.

An Account of Cholera Morbus Epidemic in India in 1817 *and* 1818. By WILLIAM STEUART ANDERSON, M.D. Member of the Royal College of Surgeons, London.

THIS epidemic is said to have originated in Bengal. We find it raging in the camp of the Grand Army under the Marquis of Hastings, in November 1817, and it has since spread to the south and to the west, apparently unaffected by the vicissitudes of climate, and but little influenced by the nature of the country, making a gradual and pretty regular progress, and visiting camps, cities, and villages, excepting only the hill-forts; its continuance in each place being generally limited from fifteen days to six weeks; seldom returning, and then in a milder form, and with less frequent occurrence. Its progress has been remarked to be at the rate of fifteen or twenty miles a-day. It reached Bombay in the end of August, having passed from Nagpour to Jaulna and Aurungabad, then to Achmendnagur, Serroor, and Poonah, although opposed by the S.W. monsoon for a great part of this course. On reaching the coast, it spread both north and south. In the beginning of October it still prevailed in Bombay, and accounts from the eastern coast mention its having appeared at that time in Nellore, about eighty miles from Madras. In many places its appearance and disappearance have been very sudden, bursting forth like an explosion of combustibles, and, as if the pabulum were all consumed, being extinguished in a similar manner.

Symptoms.—The disease commences with vomiting and purging; generally their attack is simultaneous, but the purging sometimes precedes. The matter passed is watery, and free from bile, like barley broth, rather muddy, sometimes glary like white of egg; violent pain in the intestines, with a sensation as if writhing or twisting in knots; tenesmus and spasms of the rectum, ejecting the glary matter with violence; heartburn, which the patients describe as a fire consuming their bowels; hiccup, and intense thirst; the breathing is more or less affected; it is performed more slowly than natural, or the inspirations are short and sudden, with violent pain from spasm of the diaphragm; cramps and convulsive twitches in the extremities, particularly in the legs, more violent in Europeans, and extending to the muscles of the abdomen and thorax; distention of the abdomen; anxiety and inexpressible uneasiness about the præcordia, causing restlessness and jactitation, and profuse sweats, which break out chiefly on the forehead and breast. After some hours the pulse sinks, the motion of the heart becomes fluttering, and soon ceases to be perceptible; the extremities become cold, the skin shrivelled from want of blood in the vessels, the countenance collapsed, the eye sunk and glazed from want of the lachrymal fluid, and the adnata, becoming dry, appears opaque like a film; then supervene faintings, cold sweats, and death. This is the usual course of such cases as are left to nature, or do not apply for medical relief until the powers of life have sunk too far for recovery.

In the numerous cases which I witnessed, there was no affection of the brain, although it is mentioned by some medical practitioners at other stations. They made no complaint of the head, and all retained their senses to the last.

In some cases there was no vomiting or purging, and in many these symptoms recurred only once or twice; but the other more formidable symptoms proceeded and proved not the less dangerous. This is the Cholera sicca of Sydenham. In one case, a dissection of which is subjoined, the circulating system was not affected, the pulse continuing in the wrist, and the extremities warm, nearly until death, which appears to have been caused by spasm of the diaphragm and muscles of the larynx, producing asphyxia.

Among the natives, the spasms in the voluntary muscles very often do not occur at all, and generally are much less severe than among Europeans, who are described to be attacked with violent convulsions of the whole body.

There occurred in several patients, in the last stage, an intolerable itching, or stinging sensation, in the skin, all over the body, which did not cease till death. Persons of both sexes, all ages, and every condition and cast, are liable to be affected by this disease. It has been remarked, that those who are badly clothed, and ill fed, or whose occupations expose them to the inclemency of the weather, are more liable to be affected than others. It must be admitted, that a much greater number of such people have suffered, but it should be taken into account, how much more numerous this class of the population is; and if due regard is had to this circumstance, I am of opinion that the observation will not carry so much weight with it as at first sight it appeared to have.

The disease, in children, is milder, and more easily conquered than in grown up people, and it is more violent and obstinate in the plethoric and irritable European than in the temperate native, whose constitution is very opposite.

On dissection we discover nearly the whole blood of the body collected in the large veins; the spleen is gorged with blood; the liver is distended, and, being cut into, pours out black blood from every point; the veins on the stomach and mesentery appear as if injected forcibly, and the minute vessels on the intestines have their ramifications displayed by their coloured contents, which casts a general tinge on these viscera similar to what is observed in an incipient inflammation.

The gall-bladder is full of bile; the vesica urinaria empty and contracted. There is no secretion into the cavities either of the peritoneum, pleura, or pericardium, so that the surfaces of the viscera are dry, and there is generally also congestion of blood in the lungs.

The stomach contains a considerable quantity of muddy water, and the intestines are distended with flatus, excepting the colon and rectum, which are generally contracted and thickened.

Some have described the viscera as in a state of inflammation, but subsequent observers incline to the opinion that the appearance is produced by congestion, as will be seen by reference to the subjoined dissections. It has been said, that this disease should not be called Cholera morbus, and the reason assigned is, that it does not agree with Cullen's definition, in so far as the matter evacuated from the stomach and intestines is not bilious; but it certainly admits of a question, whether this is a sufficient distinction, or should not rather be considered the effect of an aggravated form of disease, producing such a spasmodic contraction of the duodenum, as to prevent the flow of bile through the ducts. It has been alleged by several persons that bile is not secreted. But on dissection it has been found that the gall-bladder is full of bile, which renders it probable, that retention, and not suppression, of the secretion, is the cause why this fluid is not found in the evacuations. Cullen quotes Sydenham, and the disease described by Sydenham as epidemic in 1669, agrees very accurately with that under our consideration. There is no mention of bile in the description given by Sydenham, although his translator or annotator has thought proper gratuitously to infer that the evacuations were bilious, which is not warranted by his author. On the contrary, by his mention of the Cholera sicca, the opinion of Sydenham is shown that bilious evacuations were not at all necessary to constitute the Cholera morbus legitima.

The term spasmodic cholera has an appearance of impropriety, because, as cholera is of the order spasmi, it implies a repetition, as if one were to say spasmodic tetanus.

By whatever name it is called, there can be no difficulty in the diagnosis ; "facile cognoscitur," as Sydenham says ; and the only difference between this and the epidemic of 1669 is such as might be expected from the difference of climate, an aggravation of symptoms, and, instead of being confined to the month of August, its ravages have been continued throughout the year ; but it is in the month of August that the climate of England most resembles that of India ; and its disposition to terminate within the period of a month has been manifested in almost every situation in which it has appeared.

Several causes have been assigned for the production of this disease, but we are still in ignorance on this subject, as nothing that has been advanced is satisfactory.

Dr Tytler has endeavoured to prove, that it should be attributed to the bad quality of the rice used as the food of the inhabitants of Bengal ; but this opinion requires no further confutation than the observation, that the disease has spread over countries where rice is not in general use. The inhabitants of the Deckan live upon wheat and millet, which grains were not damaged during the season, but still they suffered from the disease as well as the people in Bengal.

An opinion is entertained by some that the disease is contagious, but the following considerations will tend to disprove such an idea. Medical practitioners and hospital attendants have not suffered in the manner that might be expected from a contagious disease. Under my own observation, only one out of twenty persons constantly employed among the sick suffered an attack, and no precautions were taken to prevent infection. The disease breaking out, on a sudden, in all quarters of a town, and disappearing in the same manner, in some cases, after the short space of fifteen days, without any steps being taken to cut off connection with the diseased, are circumstances quite inconsistent with the idea of contagion. Ships sailing from India would carry contagion with them, but, although many sailed both from Calcutta and Bombay during the prevalence of this epidemic, they have not suffered after leaving the ports.

Besides, unless infection could be proved in every case, if any are allowed to have been attacked without exposure to it, we must be obliged to admit some other cause, and we do not find this necessary in those diseases known and acknowledged by every body to be contagious, as the plague, scarlet-fever, smallpox, &c. From the disease pervading so generally all classes, who have nothing in common but the air they breathe, it is most generally believed that the cause exists in the atmosphere. But this opinion, though strengthened by strong argument, and supported by great authorities, is liable to an objection not easily overcome. The disease spread in a direction just contrary to the S.W. monsoon, a strong uninterrupted current of pure air from the sea, with heavy rain. Like fire amongst the grass, it spread against the wind, and passed on, after destroying those whose powers of life proved insufficient to resist it.

It has been remarked, that great vicissitudes of temperature in the atmosphere prognosticate the approach of an epidemic.—(*Vide* the Report of the Committee on the Fever of 1814, in the Southern Provinces of the Madras Territory, and the authorities there quoted.) This observation is confirmed on the present occasion. A difference of 40° was observed in the Bengal camp between the morning and noon, (*Vide* Mr Corbyn's Letter,) and I had to remark the same thing in the camp under Colonel Deacon, in Candeish, in December. We know not, however, what determines the nature of the epidemic, whether fever, dysentery, influenza, or cholera morbus.

In fact, we are, and we ought to acknowledge it, quite ignorant of the cause. Whatever it may be, it appears to affect *primarily* the system of the * eighth pair of nerves, or par vagum, producing disorder and interruption of the functions of all the organs whose actions depend on these nerves, and are independent of the brain and of volition. This affords a very probable ratio symptomatum.

The disease most frequently makes its attack after the patient has been asleep for several hours, when it may be supposed that those reservoirs of nervous influence, the ganglions, are in some measure exhausted, and unsupported by the exertions of the brain and spinal marrow, at this time recruiting themselves in sleep. The whole train of symptoms are of the nervous order, and not inflammatory. When the viscera are attacked by inflammation, its accession is not so sudden, nor are the symptoms and progress so violent and rapid.

Spasm is acknowledged to depend on a disordered action of the nerves, hence the griping and twisting of the bowels, and the obstruction to the flow of bile. The secretions have been proved to depend on the action of the nerves, and hence we find that they are either suspended or altered. No urine is secreted, and there is none of the usual exhalation into the cavities of the peritoneum, pleura, and pericardium, while the juices secreted into the stomach and intestines are quite altered. But we are not to consider the vomiting and nausea to be caused by the presence of these altered juices irritating the stomach directly, as it has been shown that these effects are rather to be attributed to an affection of "the nervous system, exciting an action which is reflected specifically on the œsophagus and diaphragm, so as to make them exert various movements, among which there are some, the definitive result of which is the compression of the stomach."—(*Vide* Account of Experiments by M. Magendie, Philosophical Magazine for 1814, p 142.) It is said, that if the eighth pair of nerves be cut, vomiting cannot be induced.

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* I was led to this explanation by reading an account of the experiments of M. Le Gallois, Mr Brodie, and M. Magendie.

The intense thirst, heartburn, and heat in the bowels, are also to be considered as deranged nervous sensations; for, on dissection, there is found to be no want of fluid in the stomach, and the tongue and fauces are not observed to be particularly dry during the disease. The matter ejected is of a watery nature, in this respect resembling the pyrosis or water-brash, which is a nervous disease.

Seeing that the diseased actions are in general spasmodic, it appears not improbable that the heart is also affected in this manner, impeding, and at last putting a stop to the circulation of the blood. The heart being more or less permanently contracted by spasm, but little blood can flow into the arteries, and is not propelled with force enough to reach the extremities. Hence the pulse at the wrist sinks, and becomes imperceptible, while it may still be felt in the axilla; but it soon fails here also, and the action of the heart ceases to be felt by the hand pressing the thorax.

The appearance on dissection strengthens this opinion of spasm of the heart, for we find as if a ligature had been put on the aorta; the mass of blood is accumulated in the veins, particularly those nearest the heart, which are the largest, and are less supported by surrounding muscular substance. But perhaps the symptoms may be easily explained, by supposing the morbific cause to act on the nerves of the heart, destroying its irritability, and rendering that organ insensible to the stimulus of the blood.

Animal heat is proved by Mr Brodie to depend on nervous energy, and therefore the coldness of the extremities is another proof of the disease being essentially a nervous affection.

This obstruction to circulation is the most usual cause of death in this disease.

The organs employed in respiration are oftener and more severely affected in Europeans than in natives. The spasm of the diaphragm is often very violent, so much so, as of itself to cause death in some cases.

From the connections which exist between the par vagum and nerves proceeding from the medulla spinalis, it is not very wonderful that the diseased action is in many cases communicated to the rest of the body, producing spasms of the muscles of voluntary motion ; but these are always of a secondary nature, and not essential characteristics of the disease.

In this deep distress and agony, it is wonderful to see how clear the understanding generally continues to the last, and the senses unimpaired. This tends further to show that the seat of the disease is in the nervous system of the par vagum, and the *effects produced by it* indeed very much resemble those described to result from wounding these nerves, or applying certain poisons which are known to affect them. No disease to which we are liable admits of more speedy or certain relief, if the proper remedies are promptly resorted to, before the powers of life sink, while the pulsation of the heart and arteries is still perceptible, and the heat has not left the extremities. In my own experience, which was pretty extensive, I only saw one such case prove fatal, an emaciated Brahmin, sixty-five years old. After this change has taken place, the case is very much altered, and our prognosis is very doubtful ; but this change is not limited to any specific time, and it is nugatory to say, that, " unless a patient takes the remedies within six hours after the attack, the case is hopeless." For many cases prove fatal before that time, while others, even after twelve hours, show no very alarming symptom. Recoveries are few among those cases in whom the change alluded to has taken place, and are only brought about by unremitted assiduity and exertion on the part of the attendants. No sign can be confided in to found a favourable prognostic, until the pulse is perceived in the wrist. For numerous are the instances in which the pain and spasm cease, and the vomiting and purging are checked, but in which we fail in restoring the circulation.

In ignorance of the cause of the disease, but little can be expected on the subject of prophylaxis. A high, dry, and airy spot is to be preferred, for we know that crowded towns, in low situations, have suffered most from its ravages, and in India such are the situations generally selected by the natives on account of the convenience of water Fatigue and exposure to the weather, bad clothing, and the use of crude and indigestible food, ought likewise, as far as possible, to be remedied. As the progress of the disease was very regular, it might have been possible, in some instances, partly to have avoided its influence, by moving in a contrary direction. Thus a camp, by moving towards the N.E. as soon as the epidemic appeared in it, would be saved some otherwise unavoidable exposure to its influence by every day's march. Suppose fifteen days to be the limits of its duration in one place, and that it spread at the rate of twenty miles a-day, it would reach a camp 100 miles distance in five days, and, by marching ten miles a day in a contrary direction, *i. e.* N. E. for ten days, they would be clear of its influence. Thus saving the exposure for one-third of the time they otherwise would suffer by remaining stationary.

Worms irritating the intestines have often proved an exciting cause to the disease, and the cases were very numerous in which lumbrici were brought up during the attack. Probably if they had previously been removed by anthelmintic medicines, the disease might have been avoided.

In the treatment of this disease, our exertions must be turned to combat its influence by such remedies as are known to produce a contrary effect. In the present state of our knowledge, all we can do is to combat the symptoms.

Since the days of Hippocrates, anodynes have been considered the first remedies in this disease, and of them the principal is opium, which has justly been designated by Sydenham to be the ancora sacra. It allays nervous irritation, sooths pain removes spasm, and checks the excessive evacuations, at the same time rather increases and excites the circulation of the blood. This is what we wish, and, in the generality of cases, particularly in natives, opium is the only remedy required, if administered early, and in quantity proportioned to the violence of the symptoms. It is a remedy within the reach of every person in India, and extensive experience has proved it worthy the appellation of a Dei donum, given it by Mr Coates in his Advertisement to the natives of the Deckan, published in the Mahratta language, which, by teaching them to cure themselves, by a cheap, easy, and speedy process, was fraught with greater and more extensive benefits than the most laboured discussions and medical regulations, or the more expensive hospital establishments,* which are necessarily limited by the small number of medical attendants procurable in extensive districts, who cannot in general afford their aid with that promptitude necessary to insure success in their treatment; a circumstance which renders the best regulations of comparatively trifling efficacy.

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* The government of Bombay erected temporary hospitals, and appointed attendants to them; but the natives were very averse to enter them, and I was informed of some that had not an admission.

All practitioners concur in the necessity of administering opium in this epidemic, and only differ in their opinion with regard to the extent of its efficacy, and the share it has in producing a cure, when accompanied with other remedies.

One thing is certain, a cure cannot be expected without opium, which is more than can be said of any of the other remedies, whereas opium alone given early, and in sufficient quantity, is as effectual in this disease as any other specific in any disease to which we are subject. *

The dose recommended is from 3 to 5 grains of opium, or from 60 to 100 drops of laudanum. If rejected, it should be repeated without delay, and it may be used in enema in larger quantity, as 10 grains opium or ss. laudanum in ℥iv. of congee or mucilage. Mr Peyton well advises, that it should be repeated according to its effects and the violence of the symptoms, without regard to the quantity. The cure should be followed by a mild laxative to remove the torpor produced by the opium. But there is danger in administering the laxative too early, as it is liable to excite irritation, and bring on a return of the symptoms; on which account it has sometimes been necessary to resort again to the opium. But this will not be the case, if an interval of ten or twelve hours is allowed to elapse from the cessation of spasm ere the laxative is given, when it will operate safely; and in this way it will be found, that there is no danger, as apprehended by Sydenham, from checking the disease *in limine*. The purgatives found most suited to this case are jalap and crem. tartar., rhubarb and magnesia, or castor oil. Mr Coates recommends the last, being easily procurable, and advises as a bitter, to restore the tone of the stomach and intestines, the bark of the Neem, (Melia azadirachta,) which grows in every village.

The beneficial effects of opium prove the disease not inflammatory. Calomel has been much in use in this epidemic. It was recommended by Mr Corbyn in doses of 15 or 20 grains, as a sedative along with the laudanum, under the idea that the disease is inflammatory. This practice was recommended under authority, and has been very generally followed, its success being in most cases attributed principally to the calomel. The following are the arguments used in its favour:

(Mr Whyte.)—" The practice I followed was that recommended first by Mr Johnston, and since by Mr Corbyn, in which the corner-stone and sheet-anchor is calomel, in a large dose, of from 15 to 20 grains in an adult, according to his strength; and I have had the most manifest proof, both in this disease and formerly in severe fever and dysentery, that calomel, in this quantity, acts as a powerful sedative, often allaying vomiting, and soothing uneasy sensations, when no other medicine will produce such beneficial results. I have in no case left the cure alone to the usual anodynes and antispasmodics; so that I am not able to say, from my own observation, what would be the result in such a case, and I have felt little inclined to do so, in consequence of the indifferent success Mr Curtis met with from the use of those medicines. I have always combined the calomel with opium, sometimes the extract sometimes the tincture, and usually with a few drops of essential oil of peppermint."

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* Dr Morgan mentions having heard an essay read at the Institute at Paris, on the cure of Cholera, by large doses of opium.

Mr Corbyn gave 15 grains calomel, which he dropped on the tongue, and washed it down with 60 drops of laudanum, and 20 drops of peppermint in two ounces of water. He says, "calomel in a dose from 15 grains to 20 is a sedative, allays vomiting, removes spasm, sends the patient to sleep, and produces one or two motions."

The Medical Board at Mádras concur with the authorities in Bengal in recommending calomel, but do not mention with what view. They advise the laudanum to be given a short time before, to quiet the stomach, and enable it to retain the calomel. From this it must be understood that they do not conceive the calomel to have the power of checking vomiting.

The efficacy of calomel, however, is by no means universally admitted. Mr Henderson and Mr Radford (in an official letter, dated Camp Kurrar, 14th August 1818) write, that, "In following the plan laid down by Mr Corbyn, we found the calomel frequently rejected, unless preceded by an opiate, and even then it seemed to produce vomiting of itself.

"When the calomel and laudanum were often rejected, it was found advisable to omit the calomel altogether, and trust to other means. Frequent 15 and 20 grain doses were evidently injurious, and latterly we never gave more than one dose, provided it remained on the stomach. How far that one dose contributed to the cure, I am unable to say, but it is certain that the natives sometimes recover from the attack from the use of opium alone."

In a subsequent letter, (dated Kurrar, 20th August,) the same gentlemen remark, "that they treated several cases lately, with the omission of the calomel, by opium, venesection, and the warm bath, and have been invariably successful when the cases were recent."

Mr Shippee (Satarah, 24th August 1818) writes: "The practice I first adopted was as nearly as possible to follow Mr Corbyn's plan; but finding the calomel not always successful, I then gave large doses of laudanum with peppermint, and if the patient was not old or too much exhausted, bleeding was found of great service."

Mr Warner writes: (Napuck, 2d September,) "At first I followed Mr Corbyn's plan, but of late have used no calomel, finding that I was as successful with the opium as when I joined it with calomel, when given liberally at first."

It frequently happens that the first dose of laudanum is rejected, whether it be combined with calomel or given by itself; but more frequently if the calomel is given than when it is omitted. A second dose given a few minutes after will generally be retained, whether it consists of laudanum alone, or is combined with calomel. But it sometimes happens that this also, and even a third dose, is rejected. This ought not, however, to deter us from an immediate repetition of the dose, omitting the calomel, and it calls for the exhibition of opium in form of enema. When the stomach will not bear laudanum in the form of a draught, with or without calomel, it can be often tranquillized by a few grains of opium, suspended in syrup, (Mr Wallace's plan,) sucked in gradually; and this succeeds after the more bulky form of a draught has repeatedly failed, even though aided by the boasted sedative powers of the scruple dose of calomel.

When the calomel did remain on the stomach, I could observe no effect produced by it; but the case went on just as those in which the cure was trusted to opium without such combination.

The calomel did not produce any evacuation, so that it was still necessary to follow up the cure by laxatives after the spasms were overcome. In some cases salivation came on, which must be considered a disagreeable occurrence, for the specific effects of mercury neither prevent nor cure the disease; patients having been known to suffer a fatal attack while under a salivation.

The only benefit I have been able to observe from the exhibition of calomel is the destruction of worms, which, indeed, occur frequently, particularly among the younger patients. On this account alone, one dose might be advantageous, but there appears no rational indication for its repetition.

The idea of its being able to " restore the balance of circulation" (Mr Whyte's Letter) is entirely imaginary; and if the "energy and action of the heart are considerably diminished," (Mr Corbyn's Letter,) what are we to expect from a sedative? Bleeding has been much recommended, and it has been proved by experience to be productive of the greatest benefits, particularly amongst Europeans, where the symptoms of general spasm run high. It is mentioned in the notes to Swan's Translation of Sydenham's Works; but I believe the chief cause of its adoption in the present epidemic was the observation in the account of the dissection by Mr Corbyn of "general inflammation of the bowels, liver, stomach, and lungs." Being of opinion, however, that this is not an essential characteristic of the disease, I am led to account for the benefit derived from bleeding in a different manner; and I think it more probable that to its action as an antispasmodic should be attributed all the advantage derived from its use.

The following are testimonies in favour of the practice:—Mr Burrel, H. M. 65th. "On admission I bled in every instance, in general to a good extent; when universal spasms existed, venesection was carried on *ad deliquium*, the patients at the same time in the hot bath to 110°. The spasms were invariably relieved, nausea and vomiting alleviated, so that the stomach bore the exhibition of calomel in scruple doses, combined with laudanum, which doses were frequently repeated; in short, opium was given under every combination with calomel, and I believe the calomel will be found to rest on most stomachs *per se*." "From having so much duty at present, I am sorry I cannot enlarge on this topic, but have only to mention my full confidence in bleeding in Europeans. The result is most striking in our hospital, as it in every case lessens the irritation of the stomach, one of the most dangerous symptoms of the disease. Bleeding has been tried in the native attendants with the same good success."

Mr Henderson.—"Having lost one or two patients who received medicine from me, in an early stage of disease, while I trusted to calomel and opium, and observing the pain and heat of abdomen, with violent spasms,—the most distressing symptoms,—determined on trying bloodletting, conceiving the disease purely inflammatory. The result exceeded my most sanguine expectations. Every case recovered that arrived early, and several that had been eight hours and upwards labouring under the disease. In every case venesection has been adopted latterly, without regard to age or constitution; and, in my opinion, neither the state of the skin or pulse ought to deter us from bleeding. The effects of it were almost immediately relieving the pain and spasms, and when enough blood was drawn, they never returned. It checked the vomiting at the same time, and the pulse became fuller and softer. Eighteen ounces was a common bleeding, and the patients never were weakened by it. Opiates were always used in large doses, and tended to allay purging, but not capable of easing burning heat, or pain of bowels."

Many other practitioners have mentioned this practice in terms equally favourable; and especially among Europeans, it must be considered an indispensable part of the treatment. It has been even recommended in cases where the pulse had sunk, and was imperceptible, to be carried into effect by opening the jugular vein; and it appeared to me probable, that, by relieving the venous system of part of the load pressing on the heart, it might tend to facilitate the renovation of the circulation; but I have not heard that in this predicament it has, in practice, ever proved beneficial, and it is of such doubtful effect, as to have been abolished in the treatment of a very similar case, that of asphyxia, from submersion.

The warm bath is another antispasmodic remedy of great service in the treatment of cholera. As it is apt, however, to cause fainting, some precautions are necessary to prevent this effect, particularly in patients already exhausted, as such an occurrence must increase the oppression on the heart, and add to the difficulty of restoring the due circulation of the blood.

During the continuance in the bath, the vapour of ammonia should be applied to the nostrils frequently, as Celsus recommends wormwood pressed out of vinegar. The addition of ardent spirits, and aromatic stimulating vegetable substances to the water of the bath, in the circular of the Medical Board of Madras, is much recommended, with a view of exciting and accelerating the flow of blood to the extremities, without diminishing the antispasmodic effects of the bath. Fomentations can be used in the intervals of bathing, or when a bath is not procurable.

Alcohol is generally recommended, and has been used by most practitioners; although those who have adopted the opinion of the inflammatory nature of the disease, of course from theory reject it. It is an antispasmodic of great power, and well suited to the present case, from its action on the circulation of the blood. The benefit derived from it and similar stimulating substances is, therefore, a very convincing proof of the fallacy of the opinion above alluded to. Wine, indeed, is the principal remedy in the treatment recommended by Celsus, and there is no doubt that mild attacks of cholera may be, and have been, brought to a happy conclusion by the free exhibition of spirits. Tetanus has even been cured by wine. (*Vide* Philosophical Magazine for 1800.) It must be observed, however, that in cases where the nervous sensation of burning pain in the stomach occurs with severity, spirits tend to increase this symptom, and the patients are very averse to their use. In this case camphor answers much better, its effects being similar to those of alcohol, while its action on the nerves produces a sensation quite the contrary. The same remark applies to sulphuric æther, and the essential oil of peppermint, while ammonia, on the contrary, should be classed with the alcohol on the other side. These are the principal antispasmodics applicable to this case. Some will prove preferable to others in particular constitutions, therefore it is proper to vary them.

The free administration of the remedies I have mentioned seldom (and in my own practice never) failed in putting a stop to the primary symptoms, vomiting, purging, pain in the bowels, and spasms in the muscles. But it too frequently happened that patients did not apply for relief until affected with a different set of symptoms, to which the greatest part fell victims. The powers that carry on the circulation were found already sunk, the pulse had ceased to vibrate, and the action of the heart had become imperceptible.

I have now to mention the remedies that were employed in this state, though their effects were not very flattering. I have above proposed the probability of this state being caused by a spasm of the heart, but it is perhaps equally probable that exhaustion, and want of irritability in that organ, producing morbid relaxation instead of spasm, may be the true rationale of this case. It is necessary, in either case, to begin with the antispasmodic remedies already mentioned, and then to proceed to such as have a direct influence in exciting the action of the heart, and promoting the flow of blood through the vessels. With this view, sinapisms (as recommended by Celsus) should be applied to the pit of the stomach. They are preferable to blisters, as being quicker in their operation, and may be composed of mustard, garlic, capsicum, ginger, &c. moistened with the water of pure ammonia. This, aided by friction, will soon produce the required excitement, if not already too far sunk. Blisters, liquified with oil of turpentine, are also recommended; and I knew one case recovered where the patient's relations had applied the actual cautery in different points over the abdomen, thinking the sinapism which I had applied not sufficiently powerful.

Cupping-glasses are recommended by Celsus to be applied to the epigastrium, and it is probable they may conduce to the same end. He also directs frictions with warm oil, which will be useful in exciting the circulation and restoring heat to the extremities. Warm flannels will answer the same purpose; and to keep up the attention of the assistants, and induce them to continue their exertions without remission, some benjamin or frankincense may be sprinkled on the coals, when the friction cloths are made warm. With the same view, these cloths are dusted over with pounded ginger and anise seeds. It is a matter of no small importance to excite and keep up the exertions of the assistants. The friends and relations generally crowd around the patient, and they only require our directions to show them what to do, their natural affection supplies all other requisites, and renders them the best assistants we can have.

Much has been said (Mr Corbyn's Letter) on the danger arising from the attendants indulging the patient with cold drink, which he constantly calls for, to allay the sensation of burning pain in the bowels, and insufferable thirst. Yet this very article (very cold water) is recommended by Celsus in strong terms, and the observation is confirmed by the celebrated Dr Cleghorn of Minorca, on the credit of the Spanish physicians. It is impossible to reconcile such opposite statements. Under these circumstances, I am of opinion that the accuracy of the observation fairly admits of a doubt. An analogy occurs, however, which appears to me to give a probable explanation of the observation alluded to. In the case of a man mortally wounded in the body, there occurs the same symptoms of burning pain and intense thirst, which lead to exertions to obtain water to drink, when, no doubt, it will often happen that the sufferer will expire in the act of drinking. (See Lord Byron's poem of Lara.)

That panting thirst which scorches in the breath
Of those that die the soldier's fiery death,
In vain impels the burning mouth to crave
One drop, the last, to cool it for the grave.

Thinking the galvanic influence very likely to renew the action of the heart* after it had been suspended, I had an apparatus made for the purpose of trying its effect. But the workmen took a long time to make it, and after it was ready, I had only two opportunities of trying it, both of which were unfavourable, being reduced to the last extremity before galvanism was applied.

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* This was denied by the early writers on galvanism, but their opinions have been since fully refuted.

I had 100 plates of five inches in a wooden trough, and used a solution of muriate of ammonia, but the trough was badly constructed. The apparatus, however, gave a very smart shock through the whole body, and I proceeded to use it on a female patient of middle age and robust habit. Her extremities were cold; pulse imperceptible; eyes fixed and dim; countenance collapsed; she could not articulate; and it was with great difficulty she had been able to swallow a dose of laudanum ℥i. with peppermint, which, with the aid of the hot bath and a glyster containing ten grains of opium, had stopped the vomiting and purging completely; but the patient was evidently in a moribund state. The trough was placed along by the side of the patient, and one wire was applied to the foot, the other to the pit of the stomach, and sometimes to the hollow above the sternum.

After five minutes the respiration became quicker, and more forcible, and she began to feel smarting from the shocks, when the wire was raised and applied again, or moved about from one place to the other. In ten minutes she complained much of the pain. The respiration continued strong, and some pulsation could be felt at the wrist. Her complaints and screams from this time made it necessary to interrupt the operation occasionally. At the end of half an hour more she seemed very much revived; pulse strong; countenance lively; eye clear. She sat up and talked to her friends, and swallowed some warm brandy and water. From these flattering appearances I thought we had completely succeeded. I desired the galvanisation to be stopped, and set the friends to work to employ frictions with warm cloths. At this time I was called away, and returned in about half an hour, when I found all the favourable symptoms vanished; the pulse imperceptible; and she was sinking fast into her former state. The battery was charged anew, and applied as before, but it failed in exciting the motion of the heart again, and, although continued for some time, the patient remained insensible to its action, and in the course of about half an hour she expired. The battery did not operate so strongly at the time of the last application, but still gave a sensible shock to a person in health, although it was not sufficient to affect the patient.

In the other case a pile of 30 plates of five inches was used with cloth wet with dilute nitric acid, which gave a sensible shock as high as the elbows; the patient an old man in a state of great exhaustion. The effects of the pile were similar to those mentioned above. After a few minutes the respiration, which was slow and faint, became quicker and more forcible: and, after continuing so for some time, the pulse became perceptible in the wrist: but, on the intermission of galvanisation, it very soon sunk again, and could not be renewed by repeating the operation.

The temporary and partial benefit derived in these cases should operate as an incitement to further trials, which I was unable to prosecute by the disappearance of the disease at the station.

Dissections.—From Mr White, Serroor, 11th August 1818. A man of H. M.'s 65th had been in hospital for some time, on account of a chronic syphilitic complaint. He was attacked at eight P.M. with cholera spasmodica, attended with great debility, coldness, and want of pulse; was not bled, and died at three P. M. the following day, nineteen hours from the first attack. In the abdomen, the omentum was nicely spread over its contents, free from fat, and of a dark colour. The liver increased in size. On cutting into its substance, the mouths of its vessels were seen distended and wide, and poured out a considerable quantity of blood. Vesica fellis pretty full. Stomach very much so, and of so dark a colour, that, on a superficial examination, it might be thought in a state of gangrene. The small intestines, too, presented a similar appearance, and might readily give rise to a similar mistake, but both were perfectly adhesive and firm, and by drawing the finger over the surface, innumerable small veins might be seen running in every direction, as in a preparation nicely injected with wax. These intestines were moderately distended with some fluid, and a worm could be felt. What surprised us greatly was the appearance of the colon, contracted throughout its whole extent, from the caput cœcum to the sigmoid flexure, so much so, as to bring the sides of its internal surface into close contact; from this contraction of its fibres feeling thick and solid, not exceeding in circumference my middle finger. This could only have been caused by a violent spasm before death. I must now revert to the stomach and mesentery, and describe a most extraordinary and beautiful appearance—one which, I imagine, will invariably be found in a greater or less degree in every case of cholera, when the patient has died soon after the first attack of the disease, *i. e.* where death can be considered the consequence of the first attack.

Besides the small veins before taken notice of, which I considered to impart the dark colour of the stomach, all its large veins were distended with blood to a really enormous size, the circumference of the venæ breves being fully as large as a crow quill, running on the upper and lower surface in a pretty straight direction, and in other parts, by their convolution, resembling a closely woven net-work, particularly when held up to the light. The veins of the mesentery were tinged in the same manner, the circumference of many being nearly equal to that of the venæ breves. No injection, however successful, could have represented more correctly the course of the veins in these parts. The mesocolon was pale, resembling in this the contracted colon.

On cutting into the stomach, a large quantity of dark-looking fluid was found, and on emptying it, we discovered a considerable quantity of calomel lying amongst its rugæ, quite undissolved; from which it would appear that the above fluid is of a nature totally different from that of the gastric juice. The pericardium contained no fluid. The bladder was empty; indeed, all the secretions seem almost wholly suppressed, excepting those into the stomach and intestines, which are much altered in quality.

The lungs were much darker in colour than natural, approaching that of the liver; and, by drawing the finger over any part of them, the veins were made appear distended with black blood.

2. An artillery man, attacked at two A. M., admitted at six, with symptoms like those of the above patient; was not bled, and died at four P. M. following day. He had been comatose for twenty-four hours before death.

Thorax.—Pericardium empty. Several old adhesions between the lungs and pleura costalis. Lungs of a dark colour.

Abdomen.—Liver increased in size. Vesica fellis containing a little bile. Stomach distended with fluid, and of a dark colour, but none of its large veins distended, as in the former case, although all the small veins were turgid in a similar manner. The small intestines dark-coloured, and drier than natural. Mesentery not strikingly altered in appearance, but a portion of the ileum, to the extent of eighteen inches above its insertion into the caput cœcum, was perfectly black and gangrenous in appearance. We pronounced them so at the time; but, upon after reflection, I think the gut possessed too much tenacity of texture, and this appearance might have been given by extravasation, which, I believe, is difficult to be distinguished at all times from the former state. The colon throughout was much more diseased than either the stomach or the small intestines, although not so much so as the portion of the ileum just taken notice of. It appeared in a state between venous congestion and arterial inflammation. I think the former state runs frequently into the latter. Bladder almost empty. Fluid in the stomach still darker than in the former case. In the head, the veins of the pia mater distended as if ready to burst. No serum in the ventricles.

I assisted at the dissection of an artilleryman, who died after the first accession of the attack fourteen hours. He had been treated with large doses of calomel and opium, frequently repeated. He had been bled to 28 oz.; had used the warm bath; blisters over the breast and stomach; and anodyne glysters. His principal symptoms were those of spasm, particularly in the diaphragm, in the throat and neck, with severe pain along the spine, and difficult respiration His pulse did not sink till very near death, and his extremities were not cold during the attack. The following appearances were noted sixteen hours after death. *Externally.*—Livid marks along Poupart's ligament on both sides. *Abdomen.*—Blood-vessels on the omentum distinct, but not very turgid. The small intestines, from the stomach to the cœcum, were high-coloured, from the multiplicity of small red vessels, whose most minute ramifications were shown as by a fine injection. The larger trunks were not distended. The substance of the intestines sound and firm, without tendency to gangrene. The stomach and large intestines of a natural appearance, containing muddy water, and a good deal of air. Liver natural. Gall-bladder nearly full of bile. Spleen small in size, and natural in appearance. Bladder of urine empty, and contracted to the size of an egg.

Thorax.—Lungs collapsed. Anteriorly pale, but towards the spine quite black, resembling clots of venous blood. A portion of this part floated in water, on a level with its surface. An adhesion of the pleura cost above the right mamma. Heart: right auricle and sinus venosus full of blood; left side empty No serum in any of the cavities opened, and the viscera in consequence dry.

As the man was perfectly sensible to the last, it was judged unnecessary to open the cranium.

Observations.—If the appearance of the small intestines is considered to indicate inflammation, it must still be admitted that it had not proceeded so far as to cause death. The venous congestion was not to be expected in this case, as the circulating system had not been affected in the course of the disease, as remarked in other cases. The most formidable symptoms were the spasms of the diaphragm and muscles of respiration; and the appearance of the lungs was that of a person suffocated. This most probably was the cause of death.

Glasgow.

VI.

Reply to Mr BROWN. By H. DEWAR, M. D. Fellow of the Royal College of Physicians of Edinburgh, in a Letter to Dr DUNCAN, Jun.

DEAR SIR,—The communication on cow-pox, from the pen of Mr Brown of Musselburgh, which appeared in the last number but one of your Journal, seems to me to call for a few observations.

The practice of vaccination has, it must be acknowledged, often been supported by inconclusive reasonings; and Mr Brown's paper is chiefly occupied with pathological discussions which might afford, on both sides of the question, much scope for ingenuity; but the author has not touched closely on the question which is most interesting to those who have attended to recent occurrences. "What is the ultimate issue of the cases in which vaccination has been employed?" He asserts in general terms, and with a confidence which expresses his own entire conviction, that instances of small-pox among them are becoming progressively more numerous and more alarming. No prudent inquirer, however, will admit the correctness of this assertion, without a statement of evidence.

Common report often distorts facts in the grossest manner. I have always found that this was more apt to be the case in the vicinity of the places where a variolous epidemic had appeared, than in those places themselves. At a little distance, the voice of common report is, "that small-pox is extremely prevalent in such a place," (Cupar, for instance;) "that the cow-pox is no preservative against it; those who have been vaccinated are taking it in great numbers;" and, what is worst of all, it is immediately stated to be, "in many instances, mortal." This mischievous mode of connecting assertions proceeds from thoughtless alarm and other passions which prevent the propagators of such reports from making these plain and necessary inquiries: Among what description of patients is small-pox fatal? among the vaccinated or the unvaccinated? or among both? and in which class is it most fatal? For these inquiries they have not sufficient patience. They have taken the alarm, and the final conclusion which they have hurriedly embraced must be true, "that vaccination is wholly useless." The case is very different within those circles in which the epidemic has prevailed.

The greater part of the subjects have been vaccinated; and we constantly hear the old experienced people say, " This is not at all like the small-pox we had among us in former times, for now they all get through;" and in those families where vaccination has been neglected, and the patients are cut off, or evidently placed in peculiar danger, I have heard the parents reflect, in the bitterness of their hearts, saying, " they now saw how far they were in the wrong;" and that at a time when the epidemic prevailed among their vaccinated neighbours, but without exciting in them any apprehensions of ultimate danger.

It would be doing Mr Brown much injustice to caution him against such confusion in the statement of facts as I have now adverted to; but I think that he would do a great service to the public if he would take the trouble of collecting information of a more minute kind than has as yet appeared. It is not enough to have full journals of individual cases. We require accurate numerical statements of the proportions of cases of different kinds, and particularly the average both of attacks and of deaths among the vaccinated and among the unvaccinated. This might be done by any one who has a fair opportunity and sufficient time. I do not affect to prescribe a task for any individual, and only state my sentiments in this way because, where a mistake has been committed, and a wrong opinion publicly expressed, truth is much more benefited by the renunciation of the error on the part of the actual propagator, than by the best refutation proceeding from any other quarter. It would be a matter of great regret if a delusion on this point should gain credit from the authority of any respectable practitioner. The facts, so far as known to me, by no means warrant the assertion of Mr Brown. If the disappointments in vaccination wore such an aspect at the time that this gentleman published his separate work on the subject, as to authorise him in pronouncing cow-pox unavailing, they ought, if his present assertion were correct, to be now so strong and decided that no observer of common candour could entertain a doubt on the subject. This, I must say, is very far from being the case.

In the epidemic of Cupar in 1817, the comparative safety of the vaccinated was very striking. Of sixteen unvaccinated patients who were attacked, six died, or more than one in three. Dr Thomson quotes me as saying only four, or one fourth of the whole, probably from having his attention at the moment directed to the passage in my pamphlet in which mention is made of *four deaths in one family.* I received accounts of fifty-four vaccinated persons attacked with the same epidemic. Among these there was only one death; and some circumstances which I have stated to the public led me to regard this case as not a fair instance of death by small-pox.

Next year, 1818, I heard of a similar epidemic at Kirkcaldy, and went to that place to make inquiry. Had I been aware that Mr Brown believed that the cause for alarm from small-pox among the vaccinated was progressively increasing, I might have taken more pains than I did; I might have endeavoured to procure a numerical statement of the cases of different kinds that occurred in that populous neighbourhood, or in some definite portion of it by way of specimen. I found all the facts that I could hear of to be in perfect conformity with those which took place at Cupar, and I thought it reasonable to be satisfied with the general descriptions given by the respectable practitioners who had attended the patients. Among the unvaccinated the deaths were very frequent, the proportion being between one in four and one in three; but I could hear of no deaths whatever among those who had been vaccinated. Had five or six deaths taken place on that occasion in vaccinated patients who were predisposed to suffer severely from slight external causes, this epidemic would probably have been on a par with that of Cupar. In cases of that kind alone would the discussions on the genuineness of the vaccine disease have claimed our interest; and, if there is a spurious vaccine vesicle at all, these few cases might have been liable to ambiguity. But, taking these facts so far as known, along with the facts detailed in your Journal by highly respected individuals, together with such as have appeared in other quarters, I should hold myself quite unwarranted in leaning in the least degree to the conclusion of Mr Brown.

We have no evidence of the protecting influence of vaccination on the system being temporary. Those who entertain that opinion will do well to observe for the future such appearances as may serve for the decision of the question; and, before a pathological theorem of such importance can be received, the facts from which it is deduced must be very extensive and accurately related. The only fact which in the least degree countenances it, and that only when vaguely viewed, is, that for a considerable time after the practice of vaccination may be considered as established, we heard but little of any failures in this protection, whereas we now hear of them in great numbers. If these occur successively in different years, the sum total must be on the increase; but we are not fully entitled to say that the proportion of failures is now greater. Many cases of a disease resembling small-pox after cow pox occurred at a very early period, for which the advocates of vaccination apologized on various hypotheses. We have for these several years been accustomed to hear of cases in which the parents and the medical attendants have differed in opinion; the former conceiving that their children had been seized with small-pox after cow-pox, while the latter maintained that the disease was chicken pox; but, as the children uniformly recovered, the dispute turned out to be of little moment, and continued undecided. The probability is, that these were generally small-pox in a modified constitution.

In some late instances the facts have become more gregarious. The disease has assumed the form of an epidemic. This circumstance has obliged us to take under consideration a greater diversity in the bearings of the phenomena, and has given us some advantages in observing the pathognomy. On these occasions the severity and danger (if there was any) have never been observed to bear any ratio to the distance of time from the previous vaccination, and the universal aspect of the facts entitles us to trust in vaccination as a sure preservative against *death* by small-pox. This is the object that must always be chiefly wished for; and our wishes and hopes in this particular are not in the least degree threatened with disappointment. We cannot reason but from what we know; and we know not what new facts may in future times rise into view. We do not know but toothach, rheumatism, indigestion, ophthalmia, water-brash, psora, or any other mild disease, may become far more frequently fatal than it has ever been known to be since it first visited human society.

Our object is truth; and it is only to be prosecuted by steady and candid observation. Candour is too often considered as an almost universal virtue among persons of education, and the absence of it as a peculiar reproach. In all its perfection, however, this virtue is rare. It is incompatible with any degree of carelessness, and it implies a disentanglement from the influence of many biases which beset our weak nature in every direction. Appeals against it are made to our prejudices, to our habits, to our love of consistency, to our vanity, and our dread of losing our authority in society. A candid inquirer must be prepared to appear on some occasions to great disadvantage. An author who at one time publicly expresses certain opinions with energetic decision, and some mixture of triumphant contempt for others, and afterwards renounces these opinions, evinces a respectable portion of this virtue; and from such we are encouraged to expect some efforts to contribute additional correction to prevailing errors, and to reconcile discrepant opinions.

Professional men who agree in opinion differ slightly in their choice of the terms by which they should designate the varioloid disease which is incident to the vaccinated. Some call it "small-pox," or "modified small-pox," while others decline to give it any such appellation, only granting that it consists of "an eruption bearing some resemblance to small-pox," and which is "occasionally accompanied with more or less symptomatic fever." This is a mere difference of nomenclature; and each one will prefer that which appears best adapted for precluding mistakes which might be followed by practical mischief among those whose information is as yet partial. My reasons for calling it small-pox are, 1. That it proceeds from the same contagion as small-pox. The identity is analogous to that between scarlatina and cynanche maligna. 2. The eruptive fever is strongly marked, and the eruption often full, and decidedly variolous. 3. It is in no particular whatever distinguishable from the mildest of those cases of small-pox which are incident to persons who have never had cow-pox or small-pox.

It is sometimes called modified small-pox,—a designation with which I am not much disposed to find fault. But it is still more accurate to call it small-pox in a modified constitution. The source of the modification is here manifest. The causes of the various degrees of mildness and malignity which small-pox assumes in persons who have never been inoculated or vaccinated are not exactly known to us. Perhaps they arise from previous modifications of the constitution; and these may, for any thing we know, be casual and temporary, or they may be original and permanent. Or the phenomena may be owing to the different modes in which contagious matter obtains access to the system. The palpable difference between the inoculated and the casual small pox renders this latter hypothesis more readily admissible than we might otherwise grant. For our practical guidance it is enough for us to know, that, where-ever a liableness to the fatal forms of small-pox exists, or the possibility of falling into such a liableness at any future period, this is removed by vaccination. The advantage of it above the variolous inoculation does not merely consist in the delaying of the disease to a future period. But small pox after cow-pox is evidently a safer affection than the inoculated small-pox, and the deaths that occur in it are incomparably more rare. Our knowledge, however, will acquire greater precision, if numerical statements on that, as well as other points, are brought forward from different quarters.

VII.

Observations on the Advantages derived to Society from Vaccination; chiefly in Reply to the Animadversions of Mr BROWN *of Musselburgh.* By Mr DUNNING of Plymouth. Communicated to Dr DUNCAN junior by JOHN HENNEN, Esq. Deputy Inspector of Hospitals.

MY DEAR SIR,

THE accompanying letter from Mr Dunning of Plymouth, promised some time ago, has just reached me. The character of this gentleman stands too high, and his experience is too extensive, to require any additional testimonies from me. As I conceive the facts which he furnishes, and the observations which he makes upon them, to be of considerable importance, I delay not a moment in communicating them to you; and as the cases which I had the honour of submitting to the public in your Number for October last, have excited considerable interest both in this country and on the Continent, and have been particularly examined in the 27th and subsequent numbers of the Allgemeine Litteraturzeitung of Halle, I beg to solicit the early appearance of Mr Dunning's cases and comments, which so forcibly illustrate them.——Believe me, Dear Sir, your's very truly, J. HENNEN.

Queensberry-House,
April 29, 1819.

To John Hennen, Esq. F. R. S. E. Deputy Inspector of Hospitals for Scotland.

SIR,—I have just read your faithful and luminous detail of the eruptive diseases, which have lately appeared in the military hospitals of Edinburgh, with a high degree of satisfaction, and with all that interest which a person must necessarily feel, under whose observation similar cases have frequently fallen, to whom, indeed, they have been long familiar, and who, therefore, may be supposed to be not ill-qualified to appreciate the importance and value of your paper. Thirteen years ago I submitted to the public some cases, similar in all their characters to many of those which you have now so accurately given us, and drew from them then the same inferences, and came to the same conclusions, that you have at this time advanced with respect to them. That we should have arrived at the same conclusions on this important feature in the history of vaccination, without any previous knowledge of, or communication with each other, (for it is evident you had never seen my observations,) is, perhaps, the strongest circumstance which can possibly be adduced to establish the truth of them. My little publications, written under the pressure of much avocation, were too unimportant both with respect to the volume and matter of them, to find their way into Scotland, where of course you have been unable to procure them. I have taken the liberty, therefore, to forward some of them to you, accompanied with this letter. That they were written too hastily, and so far I am ashamed of them, every page bears abundant evidence; on the present occasion, however, and at a moment too, very important, I believe, to the interests of vaccination, I flatter myself that the many inaccuracies which you will encounter in limine, and I fear at every page as you advance, will not deter you from wading on a little; and I do also flatter myself that you will forgive the obtrusion from a stranger of a long letter. The fact is, that my feelings on the present question, and with respect to vaccination generally, are, I fancy, perfectly congenial, and in perfect unison with your own, and truly your excellent paper has again called them up. You will immediately see that I was more concerned about the matter, than the manner, of my little publications,—magis de re quam de verbo,—yet upon the whole you will think, perhaps, that I have not looked superficially to the question.

The imperfect cases of vaccination, and, I may add, the few perfect cases which leave the human constitution exposed to subsequent modified attacks of the variolous principle, are, there can be no doubt, derived from a variety of causes, and it is from this various source, that we meet them in so many different characters and degrees of severity. I have seen these secondary variolæ, (I believe it would be more correct to call them variolæ post vaccinationem,) probably in almost every variety of their attack. This place, the town of Dock, is certainly very convenient for making observation in medical practice. During the war, we had a population of at least 30,000 in a town of narrow limits, so that any remarkable circumstance that happened in the practice of vaccination, which for many years was more generally adopted here than, I believe, in any other town of the united kingdom of the same population, was usually soon known, and, as I had always been an active and zealous promoter of the practice, brought in most instances under my own immediate notice.

I have known the cases attack sometimes with great severity, with some delirium, as happened in your son, and with a degree of heat hardly less than that which accompanies the severer attacks of scarlatina, and sometimes with almost a general efflorescence. In some instances, I have seen these attacks followed by a numerous eruption, and seen these too, in a few instances, apparently going regularly through all the stages of a full crop of distinct regular small-pox, such as sometimes happen from casual infection, and such as I had formerly often met with in the practice of inoculation; and, in more than one instance, the eruptions were so numerous, that, at the full point of maturation, they become on the face almost every where coherent. But I have *never* seen an attack followed by small-pox of a confluent character, or attended with petechiæ, or hemorrhagies, or any other trait of malignancy, nor do I remember to have seen a single case followed by any secondary fever, after incrustation, which claimed attention; and truly, I rejoice to add, that I have never seen a case which, upon the whole, I deemed to be a dangerous one. When the eruptions in these cases are, as they generally are, few, they are very frequently surrounded with an extensive areola, and appear exactly like as many diminutive cow-pocks. I have seen many of these attacks; for, from all the preceding and concurrent circumstances and symptoms that attended them, I could not for a moment doubt the nature of the disturbance going on, followed by no eruption; and occasionally I have seen them terminate in an efflorescence only, which has sometimes been evanescent, and sometimes continued for a day or two. And hence, the attack has sometimes, I know, been mistaken a day or two, and will no doubt be again mistaken for a case of scarlatina, and probably I may have made this mistake.

When called to these cases, it has many times been a question with me, whether the attack was that of scarlatina or these variolæ, and at these times it has been a frequent observation of mine, when the attack was sometimes very severe, that I hoped the case was *simply* and *only* a case of the latter; that these being never dangerous, they never alarmed me, and thus I have many times relieved the friends of my patients from their apprehensions of an approaching dangerous disease. I believe I have sometimes been able to recognise those cases when many practitioners would not have suspected them, but truly I assume no merit on this account, because my opportunities have been greater, and my attention has been more particularly directed to this branch of medicine. But unquestionably these attacks of variolæ post vaccinationem, or vaccinated variolæ, or they may be termed the neutral results of two inexplicable actions, are, in a vast majority of the cases which occur, of the mildest nature, and excite now with us very little interest or solicitude. Finally, I have never seen a case of small-pox post vaccinationem, approaching in its severity to that of a case of small-pox after the inoculated small-pox, which fell under my observation here a few years since, in the person of a lady of considerable rank, and great respectability. After much violent eruptive fever, she was loaded with a confluent small-pox, and I judged her situation dangerous. Dr Woollcombe was called in, who also thought her to be in some danger for several days. The alarm soon brought down to us her mother from Bath, who, previously to her arrival, was quite incredulous of this fact. She assured us that her daughter, with several others of her children, had been inoculated more than twenty years before by a very respectable surgeon, and an experienced inoculator, whose name she mentioned, but which I do not now recollect; that they all had the small-pox very satisfactorily, and that this lady had the disease in every respect as amply and satisfactorily as the others. Dr Woollcombe most readily associates his name with this case. I have seen other cases of small-pox after inoculated small-pox, but these, although they were indisputable, were much less strongly marked than the preceding one.

All our experience, hitherto, having shewn that these secondary attacks of variolæ never lead to dangerous results, is it not a little remarkable that they should sometimes so much surprise and alarm the system? I have some reason to believe at present, that these attacks very frequently, if they do not generally, announce themselves more suddenly than those of primary variolæ, and I have certainly known some of those attacks, which were followed by a *few unmaturating eruptions only*, characterized by more violent symptoms than have often preceded cases of fatal primary small pox. If it be true, then, that these attacks are occasionally more sudden and violent than those of primary variolæ, and I am disposed to believe that a little attention to the subject will confirm this opinion, will it be too trifling to ask, if these circumstances may possibly happen, because the constitution, already assured against the dangerous effects of small-pox by a previous vaccine impression on it, is now proportionally off its guard with respect to any future approach of this enemy, and, therefore, do those laws of self-preservation which, we conclude, so powerfully resist the sudden introduction of the matter of any acute contagion, and more especially that of small-pox into the constitution, now feebly oppose the intrusion of the latter, divested of its powers to endanger life? And thus does this contagious poison pass, as it were, unnoticed into the circulation, suddenly excite there, as we have seen, violent but temporary fever, and exert quadantenus its specific virulence. We know that this contagion, when applied directly to the absorbents of the skin, in the process of inoculation, does not alarm and establish itself in the system earlier than in seven, eight, or ten days; that this virus, from a casual exposure to it, does not produce these effects in so short a time; and that in these respective applications of the variolous principle to the human body, several very slight precursory symptoms often announce the presence of an enemy, many days before the eruptive fever attacks with any degree of violence. I am not at this moment aware, that such circumstances usually precede so distinctly the approaching attacks of secondary variolæ—may not the case of your son be quoted as an example of this? He went to school on Tuesday morning, and you found him in the evening under the full influence of violent fever; for, although he had complained of cold and sleepiness in the morning, and had not eaten his breakfast, it does not appear that he had been at all disordered before, or, in other words, that he had been, as we say, drooping or sickening many days for small-pox. But these questions are more speculative than useful, yet they may suggest others of greater interest and utility.

That the variolæ and vaccinæ, whatever their common origin may have been, are different modifications of the same principle, I have always most steadily believed, and it is from this contemplation of them, that I have always derived my greatest confidence in the powers of vaccination. The experiment, which has now been conducted more than twenty years throughout all Europe, and all the civilized parts of the world, and even beyond these, on a more extensive scale than was ever employed before on any subject of science or art, must now, we should have generally supposed, be allowed to be both a decisive and satisfactory ordeal of vaccination. Yet there are some who are not satisfied with the experiment; who not only say, that they see *nothing valuable* in vaccination, but say also, that it is *not innocent*,—that it excites *greater severity in scrofulous cases*,—that it produces a more early occurrence *of phthisis pulmonalis*,—and that it renders *subsequent measles more severe and more fatal*,—who advise Dr Jenner as his best friends (and of course all who employ vaccination) immediately to retrace their steps, to acknowledge the adverse circumstances of his discovery, and the insecurity and danger of it, and thus virtually to recant and undo all the opinions they have advanced, and all that they have done in its favour, and to relinquish the practice for ever. *If vaccination then does not defend the lives of the subjects of it from the small-pox, and their persons from the mutilating ravages of it; if it excites greater severity in scrofulous cases; if it accelerates the attacks of phthisis pulmonalis; if it renders measles more severe and more fatal;* and in short, *if all these defects and effects of it are quite so obvious and so notorious as Mr Brown assures us*, then have Dr Jenner and the Royal Colleges of Physicians and Surgeons of London, Edinburgh, and Dublin, and a long list of other eminent and respectable physicians and surgeons, ill discharged the vast responsibility which attaches to them. They ought long since to have adopted the measures which Mr Brown, as their best friend, now recommends to them, and to have withdrawn their sanction and protection from the cow-pock. Mr Brown's positions do little less, than place them all among the *most negligent, disingenuous, and dishonourable men in the kingdom.*

No medical question of equal interest and utility had preceded the cow-pock, and yet, most perversely, by none has the public mind been more painfully agitated. When Dr Jenner first proposed this milder inoculation as a substitute for that of small-pox, and as a probable mean also of obliterating altogether the latter disease, he had of course to contend with almost insuperable difficulties. The doubts, and fears, and antipathies, and, we might on this occasion say, the rational prejudices, which the extreme singularity and magnitude of his propositions naturally and irresistibly excited in every bosom, were very soon, in very many instances, by a quick transition, too frequently observable in the human mind, converted into blind, wanton, and furious opposition. The grossest misrepresentations, the most disgusting caricatures, the bitterest reproaches, &c. &c. &c. immediately assailed him almost in every direction. In the mean time, however, Jenner proceeded with incredible industry, and a confidence with which nothing apparently but truth, and a perfect conviction of the importance and value of his pursuits, could have inspired him. He was successful, and now Time, the great arbiter of all our motives and all our actions, seems to have nearly confirmed all the essential points, and all the promised usefulness of his great discovery; yet the fulminations against it, quoted in the preceding extracts from a paper lately printed in a journal of such extensive circulation, cannot fail, even at this time of day, to occasion much misery and distraction. They will lessen the little faith of the wavering, and strengthen the prejudices and fears of all those who have reluctantly acceded to the new practice. The insinuations and assertions in this paper are bold, direct, and strong, and the whole tenor and drift of it are distressing. I am very glad, therefore, to say, and I feel it peculiarly my duty to say in my turn, in strong language also, that I believe these insinuations and assertions wholly unwarrantable, and *(I do not mean to speak offensively)* that they are not true. They are, however, of a description not to be repelled by contravening ipse dixits only.

Here it will be impossible to pass by unnoticed, and it were fair to say, unreprehended, an assertion which accompanies the inimical views of vaccination above detailed. Mr Brown adventures to say *absolutely*, —if I understand him, and Mr Brown shall speak for himself,—that *the abettors* of vaccination *cannot in the least be believed.* He asserts: "*It is impossible now to pay the smallest attention to the arguments and assertions of the abettors of vaccination, as they have been under the necessity of confessing themselves wholly in error.*" Now, as I have always thought that there is nothing in the world so beautiful as truth, "rien n'est beau que vrai," and as I trust I have not often lost sight of this impression on my mind, I must take the liberty to tell Mr Brown, that the epithet by which he has thought proper to designate the friends of vaccination is a very opprobrious one, and that the whole sentence in which it stands is a still more opprobrious reflection on the ingenuous supporters of vaccination, who will hasten to condemn and suppress it with as much industry and zeal as Mr Brown, whenever they are convinced that a longer use of it threatens the comforts and happiness of society.

I have not hitherto, I believe, in my feeble defences of the cow-pock, made use of language inadmissible in literary discussions, and I shall always deeply regret if I have done so on this occasion. While I deny, without the least hesitation, that *these abettors* of vaccination have been obliged to confess themselves wholly in error, or even to admit that vaccination has suffered materially in their appreciation of it, I am simple enough, at the same time, to believe, and men of common understandings will be simple enough to believe also, that, if *these abettors* of vaccination have been candid and honourable enough to acknowledge that they have taken some erroneous views of some parts of the subject, which their further experience corrected, they have done that which, of all things in the world, is best calculated to do them the greatest credit, and to entitle them also to *some attention.* To counteract, then, such indefensible attacks on the cow-pock, it will be necessary, and it must be the business of every medical man, whose opportunities for observation have qualified him to offer himself to the public at this time on this important occasion, not only plainly and faithfully to state his experience, but to make also, if he possibly can, such an appeal to the good sense of that public, that all may be enabled to judge satisfactorily for themselves. Under this feeling I am not aware that I can better discharge at this moment such an obligation than in some such way as follows: Let it be assumed, That were I called upon to produce, and the preservation of my life depended on my producing them, a dozen children who, in the common meaning and acceptation of the word, can be said *to be marked with the small-pox*, which had attacked them after vaccination, I most solemnly declare, as if I were on my oath, that I should be very much at a loss to point out half that number; and that, if called upon on the same conditions, to point out a single individual, who had suffered from the same cause, *any mutilation, loss of, or impaired, sight, any injury of his constitution*, or who has now to complain of the least deformity of his person, I repeat with the same solemnity, that I believe my researches after such an one would be entirely fruitless; and, farther, that if so called upon to publish all the memoranda *of all the fatal cases of small-pox* which have occurred in this neighbourhood after vaccination,—teneatis risum? When, therefore, it is known that these towns and their immediate neighbourhood contain a population little short of 100,000, and that the practice of vaccination has, as I have said before, been probably more extensively employed within this range of population, than, *cæteris paribus*, in any other of the united kingdom, then these solemn asseverations, I presume, can hardly be received as trifling and worthless evidence of the protection which is assured to the human constitution by vaccination against the small-pox. Be it what it may, I depose and present it with unspeakable satisfaction, and heartily do I wish it may be so received. Let us pause a little here, and ask, with soberness and deliberation, whether, if the small-pox inoculation were carried on to the same extent for twenty years in this neighbourhood, it would, at the expiration of it, furnish us with similar results, or with results a thousandth part so favourable?!! There is no candid man, I believe, on earth, who will undertake to answer this plain appropriate question in the affirmative. *Two children have been destroyed by this process in this town within a few weeks.*

The preceding views and propositions are plain and simple; they are honest too, and are easily understood by every body. I do not know how common sense and common honesty can distort or disturb them. I could here expose a sad detail of afflicting events which formerly fell under my observation in the inoculation for small-pox, but I shall spare myself the painful task of writing, and you of perusing, such melancholy and loathsome details, and shall only add a few circumstances which arose in these respective inoculations in my own family. My youngest daughter, before Dr Jenner had announced his discovery, was inoculated with all the precautions which an affectionate parent, and a surgeon of no inconsiderable experience, could premise. She had the most confluent disease of the crystalline character over the whole body. When she was in all probability dying, she was most fortunately snatched from the grave by being fomented over the whole surface with emollient decoction, and immediately afterwards wholly enveloped in immense cataplasms of the same description ;—I say when she was, in all probability, dying, because that general collapse and extreme jactitation had taken place, which very often immediately precedes the close of fatal cases of small-pox, and from which I had never seen a recovery :—she almost immediately became calm, and soon fell asleep in the soft medium of a poultice. * She was a sweet child. She is now another example of the sad effects of the inoculated small-pox. Such were the small-pox in my family. The following minute was printed in my " Minutes of Experiments," published in 1804 : " At a time when a thousand reports directly subversive of vaccination had crept into circulation, and were fast taking possession of the public mind, it appeared to me indispensably necessary, unless we relinquished the practice altogether, to adduce some facts to counteract them, which would at once appeal to the bosoms and understandings of every one. For this purpose, my infant, who was ten months old, and whom I had vaccinated eight months before, was taken this day, January 23, 1802, into the small room of a poor woman, Elizabeth Cock, residing in Cross Street in this town, and was there inoculated by my neighbour Mr Little, with matter taken immediately from this subject. An early and considerable local affection on the arm took place, and gradually progressed till the seventh or eighth day, when it began to shade, and in a few days dried off. This inoculation was neither attended by the slightest constitutional effects, nor by the smallest vestige of small-pox, and my babe is now, thank Heaven, in the most perfect health. I know not that a stronger test than this can be imagined, and yet I do not at all fear that those who know me

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* It will not be out of place just to mention here, that my eldest son, a few months since, saved a child in this town under a confluent case of small-pox, and nearly under the same circumstances, by similar means. He probably saved her life, but he could preserve but one of her eyes.

will most readily and fully acquit me of any want of parental feelings. Truly no other motives than an *unshaken faith* in the protecting power of the cow pock, an imperious sense of professional duty at this time to the public, (I had been most actively engaged in the practice,) and a most anxious wish to assist in establishing the deserts of a discovery which appeared to me incomparably the most important and consequential in the annals of medicine, could have carried me to such a length of experiment."

My son, who was the subject of the foregoing experiment, has just passed his eighteenth year, and is in perfect health. Since the decisive experiment with the small-pox, he has many times, at different periods of his life, the last time a few months since, been tested with the cow-pock, and he must have been very often exposed casually to the small-pox in this town, where this dreadful disease hardly ever disappears. A little more than a year ago he went with his brother and Mr Silcock, surgeon of the 90th regiment, at that time in this garrison, into the small-pox ward of the military hospital, where a woman had just died of this disease, and where there were several other miserable objects of it. From this circumstance, and from the uniform results of all the tests with the cow-pock, I have every reason to conclude, that he is at this moment as unassailable and invulnerable by small-pox as he was immediately after the primary vaccination. In these two cases of my two children, the features of the variolous and vaccine inoculations are very strikingly pourtrayed and contrasted. Compare, for a moment only, the afflicting situation of my daughter under the former, with the complete panoply that has been so happily extended over my son by the latter, and then say, how much reason I have to condemn the one and to advocate the other.

While writing this, April 20th, 1819, I have under my observation a young gentleman whom I vaccinated more than nineteen years ago, April 9th, 1800. He has had smart fever two or three days, and has now a few small pustules about him, not exceeding half a dozen. They are surrounded with that areola which I alluded to above, and look like as many diminutive cow-pocks. The degree of disorder in this case evidently does not correspond with the length of time which has elapsed since his vaccination. There was in this instance a broad, florid, and apparently deep sulcus along the middle of his tongue, hedged, as it were, on both sides with a thick, raised, silvery fur. It has continued three days, and is now most rapidly disappearing, and with it, as rapidly, every disordered feeling. We meet with this state of tongue, I believe, sometimes in chronic affections of the stomach, and some of the other viscera; and, I think, sometimes too in certain cases of scarlatina and sore throat. I do not remember to have before remarked this appearance of tongue in these cases. It may have escaped me; and, therefore, I cannot undertake to determine if it be at all connected with this state of things. I have deemed it worth while, however, to advert to it, for if it is hereafter occasionally noticed in these cases, it will assist us in detecting them.

With these premises before me, will it be too much,—will it be extravagant, to say, that I believe vaccination needs no better foundations than it rests upon at present; and that, if it always maintains its present ground, it will for ever remain a signal honour to this country, where it seems by a particular providence to have been first directed; and the name of Jenner, if the preservation of human life be the measure of desert, will necessarily in every age take place of all the illustrious worthies who adorn the history of medical improvements.

To advert once more to the hostile views of vaccination which have issued from Musselburgh, for I fear these will bring much mischief in their train, let me very seriously ask, whether, before we go the whole length which is the immediate object of them, it would not be well to institute another experiment of an opposite character to that we have seen during the last twenty years, and to wait the result of it. Before we finally relinquish it, let vaccination be every where wholly discontinued for the next seven years. Let variolous inoculation be immediately substituted to the same extent. Let a register and history of all the prominent events which happen in these inoculations during the whole of this period be most carefully preserved, and, at the expiration of it, let them be carefully collated with all we have seen, and with all that is recorded to have happened in the whole practice of vaccination during the last twenty years,—shall we not hear a thousand Rachels mourning for their children, who will not be comforted, and vaccination! vaccination! vociferated from one end of Europe to the other? Then would the galled jades go wince, who violently opposed its introduction, and afterwards virulently assailed it in every stage of its progress. Once more,—Mr Brown says, " I feel much pleasure in finding my observations were well founded, and that *the sentiments and opinions* they suggested *are likely to be confirmed.*" If I understand Mr Brown, I sincerely wish that I have mistaken his meaning, he appears to me to say, he feels much pleasure that the unfavourable opinions he held of vaccination in 1808 are likely to be confirmed, and that they will soon lead to the adoption of necessary measures for discarding it. Good God! does it really gratify Mr Brown that his *random conjecture* may happen to be true, when the source of his gratification must necessarily involve the *death of many thousands*, and the most *distressing solicitude of ten times ten thousand of his fellow creatures?* Where, in the page of history, are there many examples of more devotedness to the pride of opinion? or of more remorseless sacrifices to it? These remarks, which have arisen in my defence of vaccination, have not been written in the spirit of recrimination, although Mr Brown has not hesitated to call my neighbours and myself the abettors (Dr Johnson says this means connivers) of deliberate imposition on the public, and, when cautioning that public against us, to tell them, that all that *we assert* is not entitled to the *smallest degree of attention.*

But probably, Mr Brown has, very fondly, taken too much for granted, and prematurely congratulated himself on the confirmation, weight, and success of his opinions and suggestions; certainly with respect to their influence on the minds of the editors of the Edinburgh Journal, we find he has, for at parting with him, they endeavour to set him right, and assure him, that they differ with him "on some of the *most important*, *especially the effects likely to result* from *the introduction and continuance* of vaccination." How widely different, then, do the editors of the Journal and Mr Brown think of vaccination!! If vaccination should ultimately, or early fail us, we cannot say that Mr Brown was a false prophet, nor deny, that he will justly be entitled to as much celebrity (to less honour perhaps) for his acumen in predicting the *fall* of vaccination, as the authors of some almanacks are for that of predicting the *fall* of snow, or the other accidents of weather;—they are all occasionally right—by chance.

No fact serves to place the superiority of vaccination in a more striking point of view than the following. In some families there is that high aptitude to small-pox, that, whether the subjects of it be exposed to its dreadful operation, by casual or artificial infection, some of them inevitably fall miserable victims to it, and others only escape with mutilations, loss of sight, or with a general wreck of the constitution. Here the *cow pock triumphs indeed*, for although perfect vaccination does not sometimes entirely subdue that extreme susceptibility, yet by effecting certain inexplicable conditions of the constitution, it completely disarms future attacks of small-pox of their dangerous violence and malignity; or, in other words, by modifying, or, as you have more lately termed it, by neutralizing these effects, it never fails to preserve the persons, and save the lives of these subjects.

Upon the whole, the general result of this experiment has been to prove, I think, that the vaccine, when perfectly introduced into the human constitution, renders it, with a few exceptions, arising from causes which probably will always be inscrutable, ever afterwards unassailable or invulnerable by small-pox; that it *always* protects it *against the fatal attacks* of this disease; that it furnishes us at least with all the advantages, without any of the numerous mischiefs of variolous inoculation; and that it insures to us, in all probability, a mean, if generally adopted, of abolishing the variolous principle in this country, and possibly throughout the world. If we admit that a fatal instance happens, either immediately from vaccination, or from the variola post vaccinationem, once in ten thousand cases, and I am not aware, that the proportion has been by any means so large, this admission cannot be said to affect the general validity of this general view of the practice of vaccination.

The views and opinions advanced by yourself, Mr Bryce, and Dr Thomson, in the last number of the Edinburgh Journal, with respect to the modifications of the variolous principle, which occur now and then after imperfect vaccination, and in a few instances, from some constitutional anomaly, after perfect vaccination, will, it may be, give some importance to my little volumes, which did not attach to them before; as, however my appreciation of these occurrences so long since was the result of the closest and most dispassionate investigation, undertaken, too, sometimes not without apprehensions for the fate of vaccination, I always felt assured, that the time was not distant, when men of correct observation and judgment would come to the same conclusions on this subject. That period is now probably arrived. The powerful, triumphant, and united testimonies in favour of vaccination, given in the 56th number of the Edinburgh Journal, (I hope it will long be quoted and remembered as a celebrated number of that very respectable Journal,) were the result also, on your part, of similar investigations of similar premises. Such testimonies cannot fail soon to remove from the public mind much of that agitation which has lately arisen in your neighbourhood, and many other parts of the kingdom, from these instances of modified variolæ, and I trust also, that such testimonies, when generally circulated, will lead to the adoption of some more efficient, and even legislative means of establishing, on broader grounds, the practice of vaccination, and, at the same time, of suppressing variolous inoculation altogether.

This latter practice was for some years wholly discontinued in this town, and nearly so in the neighbourhood, but the occasional occurrence of the cases in question,—the hasty representation of them by some, and the wilful misrepresentation of them by others, have, I lament to say, too generally re-established it. Still, however, there are many practitioners who continue to vaccinate exclusively, and whom I may, I believe, undertake to say, no reward or remuneration can, under existing circumstances, tempt to dip a lancet in variolous poison. Need I add, quorum fui?

With respect to the causes of variolæ post vaccinationem, I will just observe, that inattention to some circumstances in conducting the process, I judge to be a frequent and fruitful source of an imperfect cow-pock. It would be, I believe, an important rule, never to insert lymph from a vesicle before the formation, and never after the least declension of the areola. The cow-pock at this period of the process, it must be concluded, has acquired all its most active and specific properties, and, certainly, such properties must be best calculated to convey the strongest and most perfect impression, and, therefore, as much experience has now taught us how great a desideratum it is to secure such impressions of it, there can be no good reason, truly no reason at all, given for employing lymph taken at any other stage of the vesicle. It is important, too, in several points of view, to establish a cow-pock on each arm, and always to leave one of these untouched.

You will forgive me, I hope, if I detain you a little longer, to shew you how early in the practice I ventured to suggest to Dr Jenner, the modifying power (you call it the neutralizing power) of the vaccine on the variolous susceptibility in those constitutions where it cannot entirely subdue it. After some letters had passed between us on this part of vaccination, in one now before me, dated May 1st, 1805, he says, " Whether your conjecture respecting the security given to the constitution from the imperfect action of the virus, may or may not be just, is a point which I will not now contend with you;"—and in another, dated July 29th, 1805, again on the subject of imperfect vaccination, in the most handsome manner, and with that candour and liberality which have never deserted him in the course of the arduous investigations, and of a correspondence of an unprecedented extent, to which the novelty and importance of the question he had brought forward subjected him, he observes, " From what you have already said and observed on the subject, I inculcate every where the propriety of observing greater precaution, *and I found it entirely on your observations* in conducting the process of vaccination. I recommend invariably two pustules, and that one should remain unmolested;" and looking to the same objects, he farther writes, " I recommend, too, that no great number of persons be inoculated from the same pustule, from a suspicion, but it is a mere suspicion, that the vaccine fluid, rapidly called into existence, may not possess precisely the same powers as a fluid slowly formed, and in the state we find it in the unbroken pustule;"—and afterwards, adverting to the same subject, in a letter to Dr Willan, dated February 23d, 1806, and printed in an appendix to his valuable work on vaccination, he says, "It strikes me, that the constitution loses its susceptibility of small-pox contagion and its capability of producing the disease in its perfect and ordinary state, in proportion to the degree of perfection which the vaccine vesicle has put on in its progress, and that the small-pox, if taken subsequently, is *modified* accordingly;" and then adds, "this opinion was first published by Mr Dunning of Plymouth."

And now, Sir, if you have accompanied me thus far, you must go along with me a little farther, and not deny me the vast pleasure I shall enjoy, in transcribing the strong testimonial in favour of vaccination, which, after the most deliberate and analytical examination, I believe, of the late events in Scotland, you have given us. I shall then subjoin a few extracts from my own paper. It will only be necessary to collate these for a moment, to shew what I set out with,—That we have respectively and independently arrived at the same conclusions, on a *feature so prominent* in the history of vaccination, that a full and fair appreciation of it, in all probability, almost involves even the existence of the practice. You say, "After the most mature consideration, I must explicitly avow, that nothing has occurred in these cases, which has in the smallest degree shaken my opinion of the great and pre-eminent importance of vaccination, whether we view it as a preventive of small-pox in a vast majority of cases, or as a most effectual neutralizer of its malignity in the comparatively few instances in which, from some peculiarity of constitution, or some anomaly in the process, hitherto not fully developed, it has failed to afford this permanent security;" and then you add in capitals, "On the contrary, it appears to me, that the whole series of cases which I have given *present the most triumphant evidence in favour of vaccination, and place, in a most conspicuous point of view, the infinite advantages to be derived from the process when judiciously conducted.*" As early as 1804, in my detail of events which happened at the village of Stoke, &c. I took occasion to say, "That I expected these occurrences of small-pox, of a *modified character*, under certain circumstances after vaccination, but thank God, that I did not fear them; that after the most *deliberate appreciation* of them, they served to *place the value of vaccination in a stronger and more prominent point of view;* and that this view of it would, at no distant period, challenge and command the attention and belief of every man of reflection and humanity;" and again, in another paper, "Further circumstances," &c. I observed, "that, looking at vaccination in all its effects and consequences, and *deliberately and severely judging them*, I subscribe implicitly to a belief that the subjects of imperfect vaccination who may be liable to a *modified small-pox*, entirely possess *all the advantages* derivable from the inoculated small-pox, *exclusively of any risk* of those afflicting and fatal events, which so frequently attach to, and follow the latter practice." And I rejoice to add now, that I entertain at this moment the same opinions; and that I am authorised to say, that these are the opinions of my medical friends in this neighbourhood.

At the risk very likely of a further laugh against me, I shall quote myself once more from "Further observations." "Can it be now a question, whether the prophylactic powers of the vaccine against the variolous principle is a law, or the exception of a law in nature? that is, whenever it gives security to the human constitution against the small-pox, is this effect of it an accident and an exception of the laws of nature? Certainly this cannot long remain a question. Does nature any where, or on any occasion, so oscillate between two extremes, as to leave us long in doubt as to what she means, or where she points? Such is not assuredly her usual march; such are not her ordinary economies." The question now has been more than twenty years before us, and vaccination, upon the whole, keeps her place. Mr Bryce, in his very excellent paper, has gone along with us in his corollaries the same lengths, drawn the same strong conclusions, and placed, in a most valuable point of view, the present state and deserts of vaccination.

Your pertinent questions will, no doubt, elicit much amusing and useful information from some ingenious men; but you know, and I know, that, in cutaneous and eruptive diseases, anomalies and non-descripts will now and then arise, which perhaps no human sagacity or investigation will ever be able fully to explain. You perceive, then, that my mind, in a general view of the subjects before us, is pretty much made up.

Having formerly been extensively engaged in the practice of variolous inoculation, and having, since I relinquished it (I hope for ever) eighteen years ago, vaccinated, I believe, nearly as many subjects as any individual in the united kingdom, probably at least as many, I am free to acknowledge, that, when speaking of these two inoculations, and contrasting them, I may possibly assume a tone of some confidence. Become, as it were, identified with the interests and fate of vaccination, from my daily and successful use of it so many years, it is really a source of high gratification and exultation to me, to say, at this period of the practice of vaccination, that I feel I am performing at once a social and a professional duty, whenever I am engaged with my medical friends, in recommending a general, and an exclusive adoption of it. In a note to a paper which I printed in the 76th number of the London Medical and Physical Journal in 1805, in reply to Dr Walker, who thought that some observations I had published relative to the injured vesicle, and the consequent exposure of some constitutions to attacks of modified variolæ, were entirely visionary, I mentioned a severe case of the variolæ post vaccinationem. This was among the earliest cases I had met with. I published it immediately. Mr Brown, then, cannot justly accuse me,—but in his sweeping assertions he accuses every body,—of suppressing or distorting the adverse circumstances, as he terms them, which have arisen in my practice, and to which, as Dr Jenner's best friend, he now so earnestly calls his attention. In my own practice, I have not since met with so severe a case. *It happened after she had been vaccinated* THREE YEARS. I advert to it now, because I saw the subject of it this morning, April 20th. She is a fine young woman, eighteen years old, and I found it absolutely necessary literally to inspect her face with some attention, before I could discover any marks of variolæ. The whole paper in which I inserted this case bears on the question before us, viz. modified variolæ, after imperfect vaccination.

In conclusion, although I am aware that the anti-vaccinist will immediately refer any wisdom in the remark to forebodings, or to fears already felt for the fate of vaccination, it will however be wise, since happily for us we cannot look into futurity, to remark, that if a further experience of vaccination should entirely reverse its expectation, and the small-pox should regain all its wonted horrors, which, God forbid; that, under such events, no opprobrium could possibly attach to professional men. They would feel only the deepest regret, who had zealously, and conscientiously, in their disinterested endeavours to abolish a besom, so destructive of human life, and with it also, be it recollected, a lucrative source of their income, availed themselves of an auxiliary which, in every view of it, seemed better calculated to accomplish that great object, than the aggregate of all the means that had hitherto been suggested or resorted to.

If you judge, Sir, that this favourable report from Plymouth can in any way serve the cause of vaccination, you are very welcome to dispose of it as you please. I have the honour to be, &c. RICHARD DUNNING.

Plymouth Dock, October 31, 1818.*

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* The greater part of this letter was forwarded to Mr Hennen on the 31st of October last. Having observed, in notices of communications in the Edinburgh Journal of January last, that a paper had reached the editors with my signature, I concluded that Mr Hennen had transmitted it to them, and expected that it might have been published in the following number. That not having been the case, I have availed myself of the opportunity of adding such observations as occurred to me, on reading Mr Brown's paper which has since appeared. With this explanation the letter retains its original date.

P. S.—I hope the matter of this postscript will carry with it the best apology I could offer, for appending it to a long letter. The business of it then is, to tell you, that two instances of second attacks of small-pox have lately occurred in this neighbourhood. The first was in the person of a very respectable young surgeon, Mr Hingston, living now at Plymstock, about two miles from Plymouth. This happened about a year ago. The disease was so severe, that the loss of his sight was very seriously apprehended for many days. This gentleman, I have reason to believe, means to favour the public with some minutes of his case. The subject of the second case is the son of Mr Fox, principal officer in the excise at this port. This young gentleman had the disease with some severity; he is about eighteen years old, and was inoculated for the small-pox when an infant. All the circumstances of these two cases were most unequivocally ascertained from Dr Stewart, with whom Mr Hingston served his apprenticeship, and immediately from Mrs Fox. A daughter of Mr Fox, who, as well as my recollection serves me, her mother told me, was about eleven years old, caught the small-pox from her brother; she had been vaccinated when an infant. She had a much milder disease than her brother. It follows, then, that the occurrence of small-pox after variolous inoculation are not so unfrequent as have hitherto been supposed.

I have this moment read the annexed register, and it forms a very appropriate appendix to the preceding letter. This register is printed in the Medical and Physical Journal for March.

An account of the number of deaths occasioned by the casual small-pox, extracted from the register for twenty years before the practice of vaccination, and also for twenty years since; also, the number of deaths, as reported by the parish clerks of London, &c. copied from their general bills of all christenings and burials for the same period.

	Before Vaccination.			Since Vaccination.	
	Hospital Register.	Parish Register.		Hospital Register.	Parish Register
A. D. 1779 to 1798.	1867.	36,189.	A. D. 1799 to 1818.	814.	22,480.

Decreased in deaths since the practice of vaccination was introduced—at the hospital, 1053; in the parishes, 13,709; total, 14,762.

With what grateful emotion and indescribable satisfaction must the preceding register (a consummation so devoutly to be wished) have been read by Dr Jenner, and very many, indeed, will largely and cordially participate with him. If a Roman citizen, who saved the life of *one fellow creature*, was honoured with a *civic wreath*, what honours are due to him, whose discovery has already preserved to his country 14,762 *lives in London only!!!* Was ever a *register of deaths read with so much pleasure before?*

April 24th.

There is at this time a third child lying dead of inoculated small-pox in Queen Street in this town. These three cases have happened within a few weeks.

VIII.

Remarks on the Pathology of the Nerves in Inflammation; with some Observations on Heat as a Remedy. By John Lizars, Fellow of the Royal College of Surgeons of Edinburgh, and Lecturer on Anatomy and Physiology.

> "The knowledge and power of man are coincident; for, whilst ignorant of causes, he can produce no effects. Nor is Nature to be conquered, but by submission. And that which in speculation stands for the cause, is what in practice stands for the rule." Bacon.

I have long been of opinion that Inflammation has not been properly understood,—at least, that all the organs concerned in its developement have not been duly taken into consideration; one or other has been generally adopted to the exclusion of the rest. This appears evident from the Humoral theories of Hippocrates and Galen; the Alchemical theory of Paracelsus; the Humoral theory of Boerhaave; the Spasmodic theory of Hoffman, Staahl, and Cullen; the Excitability and Excitement of Brown; the Irritation, Sensation, Volition and Association of Darwin; and the Materia Vitæ Conservata, Chordæ Internunciæ, and Materia Vitæ Diffusa of Hunter. Thus, all these celebrated men have taken partial views of the subject, which seems to have arisen from inattention to the natural operations of the living system; for, " Of all the desiderata in the knowledge of medicine," says Lord Bacon, " there remains one of more use than all the others,—a true and active natural philosophy, upon which the science of medicine should be built." But while I here comment on their theories, I am far from condemning them; on the contrary, for their labours I have the highest veneration. I am equally far from arrogating to myself any other merit than gleaning from these theories, and endeavouring to describe, what appears to me, the natural process of action in inflammation, and to detail a mode of treatment equally natural and universally applicable to every species of acute inflammation.

" Truth, in my opinion," says that most acute and philosophical etymologist, Horne Tooke, " has been improperly imagined at the bottom of a well. It lies much nearer the surface; though buried indeed at present under mountains of learned rubbish, in which there is nothing to admire but the amazing strength of those vast giants of literature who have been able thus to heap Pelion upon Ossa."

The theory which I now propose may be as defective as others. It seems to be the fate of every one, when he adopts any peculiar idea of the operation of an action, to overlook some *collateral circumstance* which should be taken into consideration. The lymphatics, for example, may be concerned in the elucidation of inflammation. * The principles which I am about to develope, have been hinted at again and again, but have never, as far as I know, been decisively stated, nor any rational treatment founded upon them. The treatment has been often employed in several inflammatory diseases, but never as an invariable practice.

From the experiments and reasonings of Hunter and others, as well as from what may be daily seen in several inflammatory complaints, I conceive that *in inflammation, the nerves are first thrown into morbid action*, and that *these, instantly exciting the arteries*, produce the train of symptoms and appearances usually evinced, such as *pain*, *increased heat*, *redness*, and *swelling*.

That this is the *order* in which these actions take place, and in which these phenomena appear, is beautifully elucidated and satisfactorily proved in acute ophthalmia; where we have *first, pain*, consequent on nervous action; whether caused by cold air or any foreign body getting between the eyelid and ball. *Secondly*, This pain is instantly followed by an *increase* of *heat*,* likewise consequent on nervous action; while this in its turn is succeeded by *redness* consequent on arterial action. Here no swelling of any consequence takes place, unless the disease become very severe. The succession of these symptoms is also manifest when the skin is stung by a wasp, punctured by a sharp instrument, or scalded with boiling water. They are equally illustrated in every acute inflammatory disease.

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* The lymphatics *must* be *materially* concerned in poisoned wounds.

That the nerves are first influenced is also proved by healthy action, for no simple operation is performed without the nerves beginning the series. The act of blushing commences in the nerves,—the act of walking commences in the nerves,—the erection of the penis begins in the nerves. Secretion is also regulated by the nerves.† The sensation of cold produced by affections of the mind, as fear or horror, proves the influence of the nervous action over the arterial. "Cold fearful drops stand on my trembling flesh." Sneezing and coughing are excited by stimuli applied to the nerves of the nose and lungs. Vomiting is produced by emetics operating on the nerves of the stomach. The tying the phrenic nerve of a dog, and the action of the diaphragm ceasing, and our power to excite the action for a time by stimulating the nerve under the ligature;—lastly, the experiments of Galvani, Volta, Sir Everard Home, Mr H. Earle, and Dr W. Philip, most decisively prove, that the nerves begin the series of both healthy and diseased action.

That *increase of heat* follows the pain, and precedes the redness, is easily proved by any one pricking his skin, when he will feel, instantly after the infliction of the pain, a sensation of warmth. If a severe blow or wound be received, there is a thrilling pain and intolerable sensation of heat at the moment of the injury. *Heat* seems to depend more on the nerves than the arteries.*

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* Although I have made heat follow pain, for there is no pain in animals without a certain sensation of heat, yet I am fully aware, that, in very slight degrees of inflammation, the sensation of heat is first felt, as occurs in the beginning of ophthalmia or gonorrhœa; but here inflammation is not completely established, it resembles more an increased healthy action.

† The interesting fact narrated by Humboldt, of the breast of a man, when stimulated, secreting milk, is illustrative of this theory. This apparently extraordinary circumstance is taken notice of by several ancient authors. Last summer my colleague Mr Allan was consulted by a man afflicted with cancer of one of the mammæ. He stated that when a boy he was in the habit of handling his breast. I saw this individual, and there was then milk in the breast.

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* Animal heat seems to be generated by several surfaces; that of the lungs; that of the alimentary canal; and that of the skin, which constitute the whole superficies of the body. Lord Bacon, when talking of the comparative heat of the different parts of an animal, states, that "the milk, blood, sperm, and eggs, are moderately tepid, or less hot than the outward flesh of the creature, upon exercise, or when moved or excited." That the functions performed by the lungs generate heat, is satisfactorily proved by the experiments of Crawford. That the alimentary canal aids in doing so, is easily explained, by considering the effects of hot stimulating fluids on the mouth, stomach, and intestinal tube, where, in the two latter, there is the expansive distribution of the para vaga and great sympathetics. The ingenious John Hunter was of opinion, that animal heat was generated by the stomach alone.

That the skin generates heat, is proved by several facts. By the chill feeling we experience when our bodies are not daily washed, and by the same sensation felt in those parts of the body which are not so frequently washed as others; "because," says the illustrious Darwin, "after immersion in cold water or in cold air, the whole system becomes more excitable by the natural degree of stimulus, as appears from the subsequent glow on the skin of people otherwise pale." Besides this glow, there is an evident blush. That part of the skin which is most exposed to the air is of a redder colour, and this redness is increased in cold weather, as we daily observe in the cheeks, arms, and legs of the country girl. It is the skin of the Laplander which resists the intense cold to which he is exposed; it is the skin of the African which moderates the overpowering heat under which he lives. The country girl feels less cold in her hands and limbs during winter than the town domestic. Our hands, in winter, are generally warmer than our feet. The poorer classes of society, from not washing their bodies so often as the higher, feel the cold in winter more severely. The Russians plunge into the snow to raise a glow of warmth in their bodies. The cheeks of northern nations are ruddier than those in southern latitudes.

This function of the skin is supported by comparative anatomy; for we find that cold-blooded animals, which include reptiles and fishes, have either a crustaceous or a scaly covering; while the warm-blooded animals, mammalia and birds, have a soft, tender skin. The torpidity of birds shows that neither the stomach nor lungs have any effect in preserving the vital principle. Here, if the skin have no effect, motion alone seems to produce it. But this fact prevails not only in the animal, but pervades the vegetable kingdom. "Insensible perspiration," says Sir J. E. Smith, "takes place, in some degree, from the bark of the young stem or branches." "The branches of woody plants, taken in the spring," says Keith, "immediately before the expansion of the bud, and inclosed in receivers filled with common air, together with a small quantity of water to supply them with moisture, developed their leaves as if attached to the parent plant; and this developement was effected solely by means of the oxygen contained in the receiver, for, in mediums deprived of oxygen, no developement took place." The stimulating influence of light on a plant is discoverable equally in the stem as in the leaf or flower. "The vigour and colour of the stem," says the same author, "are also affected by the presence or absence of light;" "the luxuriance of the branches," continues he, "depends also on the presence and action of light." Plants grow stronger and quicker when watered all over, than when confined merely to the roots. Many plants have no leaves, such as the remarkable genus Cactus; but their structure being

The *redness* in inflammation depends on the arteries, which have been carrying red blood, becoming enlarged in their calibre, and conveying more, as well as on those which have been conducting pale blood or lymph, becoming also enlarged, and transmitting coloured blood. The capillaries, and communicating vessels between the arteries and veins, and even the commencement of the veins themselves, must be similarly dilated.

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very cellular, assisted by the bark, it performs annually the functions of leaves, hardening subsequently into wood.

The healthy functions which the skin performs appear, therefore, analogous to those done by the stem and leaves of plants.

That the skin is concerned in the production of animal heat, is also proved by friction, or any other stimulus applied to the skin, exciting heat; by the cold and hot feeling experienced in fever and many cutaneous diseases; by the excess of heat produced in fever, and this being greater or less, according to the oppression of the nervous system; by the heat in local inflammation of the skin; by the coldness of the paralytic limb, the difficulty in inflaming it, and its easy participation in the heat of the surrounding media.—Besides, we find the heat to be more or less, according to the degree of the paralysis. The fact of adults having been known to exist for a length of time, while the lungs were nearly totally consumed, supports this theory. The recovery of a person from drowning depends as much on stimulating the skin as on inflating the lungs.

Those parts of the human body which are most plentifully supplied with nerves are the warmest, such as the skin, the alimentary canal, and the surface of the lungs; while those which are least supplied with nervous influence, as the cellular substance, the muscles, the ligaments, the bones, the parenchymatous substance of the lungs, liver, spleen, and kidney, are the coldest. This is corroborated both in health and disease. J. Hunter found, that the heat of the muscles, when inflamed, was trifling, when compared to that of the abdomen. The skin, when inflamed, but particularly of the fingers, feels intolerably hot to the sufferer, and even warm to the observer; equally so do the stomach and intestines. It is the extremities of nerves which are most sentient.

The experiments of Mr Brodie prove, that when the lungs are deprived of their nervous influence, no animal heat is generated, although the circulation be kept up, and the blood changed from its dark hue to its bright vermillion tinge by artificial respiration; for the animal becomes gradually colder. And the experiments of Sir Everard Home and Mr H. Earle decidedly establish, that the nerves are concerned in the developement of animal heat.

The nerves seem to possess a power inherent in themselves of acting, without communicating their impressions to the sensorium, in trifling occurrences, as we see evidenced when so slight an injury is done to the skin that the individual is ignorant of it. This *independent action* is better explained in the process of digestion, and those involuntary operations where the sensorium is not made acquainted with what is going on. This has been very judiciously supposed to depend on the ganglia of the para vaga and sympathetics. The same, I should suppose, is effected by the plexuses of the arm and leg in trivial operations. Now, this *independent power of the nerves* seems to enable them instantly to excite the arteries in their contiguity; and this is what apparently takes place in the skin, in the generation of animal heat, and in local inflammation of the skin, provided it be trifling.

Now, it seems scarcely possible that this effect could take place, without the nerves being first excited. If this be the unquestionable order in which these phenomena occur, then the *swelling* can be easily accounted for. The increase of blood, denoting the redness, will cause a tumefaction. However, a greater swelling often takes place than this mere increase of blood can produce, which seems to arise from the deposition of coagulable lymph.

Many, I have no doubt, will contend, that the explanation of the order of these actions and phenomena is of no avail—of no practical utility; that when disease exists, we have a sufficient knowledge of it, and that our aim then should be to cure the malady. But to such reasoning I have only to answer, that, on the precise and correct knowledge of the theory of any disease, must depend the treatment. It has been this taking for granted that has impeded the advancement of medicine. Thus, disease is described as it occurred to the practitioner, and his nostrums of treatment detailed; but no accurate theory of the disease is given, and how the remedies did effect, or were likely to effect a cure, is never dreamt of. "It was a good custom among the Turks," says Boerhaave, "that when they called a physician, they would not follow any of his directions, until he had first told the patient what his disorder was;" and, I trust that they equally satisfied themselves about his remedies. Were such the case in the present day, we should not have such irrational methods of practice.

I shall now proceed to show how far a correct knowledge of the theory points out the treatment; for I conceive, however satisfactorily practice may establish the treatment of any disease, yet, if we do not clearly comprehend its nature, and the operation of the remedies employed, that we still labour in the dark, and are pure empirics. "The knowledge of the operations of the whole animal," says J. Hunter, "or of its parts, when arising from a disturbed or deranged state, or a diseased disposition, are to be considered as the first steps towards a rational cure. But this alone is insufficient; the means of bringing the body to that state which accords best with the accident or disease, are also necessary, which will include the knowledge of certain causes and effects, acquired by experience, including the application of many substances called medicines, which have the power of counteracting the action of disease; or of substances, perfectly inefficient in themselves, but capable, under certain circumstances, of producing considerable effects, such as water when hot and cold, or a substance when it varies its form from fluidity to vapour."

The first circumstance in order and in importance which occupied my attention in this sketch, is the *morbid action* of the *nerves,* indicated by *pain.*

This symptom, therefore, demands our first consideration; and whatever can tend to subdue or arrest the action productive of the sensation of pain, should be employed. The consideration of this symptom, as the chief one, is authorised, by pain being our principal diagnostic in inflammatory complaints; as, for example, in phrenitis, ophthalmia, pneumonia, and enteritis. Indeed, in such diseases, I am of opinion, that every other symptom is of minor importance to that of pain; it almost alone should be our guide in such diseases. The remedies, then, which I have found most serviceable in abating pain, are—bleeding, hot anodyne applications, and narcotics.

Bleeding and hot applications evidently subdue pain, by reducing or arresting the morbid action of the nerves which produces it. "*Pain*" is the term expressive of a feeling or sensation which it is our nature to avoid, just as heat is the term expressive of a feeling or sensation produced by the action of caloric on our nerves. If bleeding subdue this sensation, it is by subduing its cause, which is generally inflammation,—always originally a morbid state of the nerves. Heat will subdue *pain,* by subduing its cause, *i. e.* by restoring the nerves immediately concerned in producing it, to a proper state.

Narcotics lessen or subdue *pain,* either by restoring the nerves concerned in its production, to a tranquil state; by operating on them in particular as sedatives; or by rendering the sensorium torpid, and thus simply preventing the occurrence of sentient perception of the morbid state of the nerves injured. But still the last mentioned effect will have its use; for the perception of injury by the sensorium keeps the whole system restless, and, therefore, must hurt a part, if it hurt the whole.

Bleeding operates by lessening the quantity of blood flowing to the brain, and hence reducing the stimulus to this organ and the nerves; and, by diminishing the quantity flowing in the arteries and veins, and thereby taking off the pressure on the nerves which are morbidly excited.* It is, consequently, the most powerful and effective remedy. This general bleeding is most particularly applicable to inflammation of the great organs or cavities.

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* The dependence of the brain on the arterial blood sent to it, is satisfactorily proved by the late experiments of Sir Everard Home and Mr H. Earle. It is a point of much consideration in the practice of medicine. We daily witness phenomena elucidating this fact; long fasting produces syncope; bleeding from a large orifice induces fainting; and when too great a quantity of blood is abstracted at a time, in an inflammatory complaint of long standing, dropsy sometimes ensues, which still further elucidates the dependence of these organs on each other.

Hot anodyne applications appear to be more peculiarly indicated in inflammations which are superficial, such as ophthalmia, phlegmon, and erysipelas. Nevertheless, in enteritis and peritonitis they are equally beneficial. In fever and pneumonia, although the anodyne remedies would render the treatment more efficacious, yet, from the great expence attending them, warm bathing must suffice. As I have stated that *warm applications only* should be used *in acute inflammation*, it becomes necessary to make a few observations on the operation of caloric and cold.

Caloric, by modern philosophers, is considered a subtle material fluid; while cold, merely the absence of that fluid. The viewing them in this light has a decided influence on our reasoning upon them; it biasses us in favour of the one, to the exclusion of the other; we look upon caloric only as the positive agent, cold being simply negative. But in my opinion, they are both positive states of matter, the one being the antagonist to the other. In this idea I am supported by Bacon, Boyle, and Newton; so that, if authority have any weight, it is entitled to as much consideration as the hypothesis of the present day. Yet, to enter into a detail of the various experiments to support this doctrine, would compose an essay itself; and, as it does not appear of primary importance to the elucidation of the present paper, I shall defer treating it experimentally until a future occasion, when I shall lay before the chemical world my reasons for reviving this hypothesis.

62° of Fahrenheit is the assumed mean of the temperature on the surface of the earth by travellers and philosophers, and comprehends the middle latitudes. Here, man, placed under the rays of a genial sun, attains the principal honours of his species; he reaches the highest point of intellectual greatness. But every step he takes to the south of this animating medium, he becomes dull and inactive, till at length, beneath the equatorial rays, he becomes slothful and indolent. The range of his ardent mind is limited; the splendid effusions of an immortal genius are fled; and, overpowered by solar influence, his body and mind sink exhausted into imbecility.

Park, in his interesting travels in Africa, when going from Jarra to Sego, says, that he grew faint with thirst, and his horse restive from fatigue. At this time the sun's heat was increased by the reflection of the sand. After travelling a little further onwards, he became so exhausted with fatigue, and so affected with sickness and giddiness, that he sank upon the sand in a state of insensibility. The country around Bubaker he describes presenting a dreary waste of sand, diversified with dwarf trees and prickly bushes scattered at intervals. The insufferable heat had destroyed vegetation, and dried up almost all the water; nature seemed sinking under its influence; the stillness of the desert prevailed. He mentions, that the heat had such effect on the cattle, that they lowed incessantly, became furious with thirst, and when arriving at the wells, they contended for the preference in so fierce a manner that several sank exhausted, too feeble to endure a struggle, and devoured the black mud in their vicinity.

In travelling to Tombuctoo, he was overtaken by a violent storm of thunder, lightning and rain, "the immediate effect of which, was to produce an almost irresistible propensity to sleep, and to make it impossible for him to refrain from lying down on the wet bundles, or even on the ground."

Maillet, Bruce, Sidi Hamet, and other travellers in Africa, corroborate these facts. They all agree, that immense tracts of country are deserted by living matter, no traces even of vegetable life are to be observed—the birds shun the torrid atmospheres, even the serpent and the lizard forsake the sands. When any animal comes under this vertical and burning sun, which seems to survey only the ashes of a world destroyed by fire, or the sands of a vast sea, it falls a victim. In a journey performed by Sidi Hamet, 300 men fell victims, and 200 camels. "For the mountains will I take up a weeping and wailing, and for the habitations of the wilderness a lamentation, because they are burnt up, so that none can pass through them; neither can man hear the voice of the cattle; both the fowl of the heavens and the beast of the field are fled; they are gone."

The hot winds of Africa elucidate still further 'from burning suns, livid deaths descend,'—as the Kamsin, the Harmattan, the Samiel, the Sirocco, and

> "The red hot breath of the most lone Simoom
> Which dwells but in the desert, and sweeps o'er
> The barren sands which bear no shrubs to blast,
> And revels o'er their wild and arid waves,
> And seeketh not, so that it is not sought,
> But being met, is deadly."

The effects of the Sirocco I have experienced myself, and shall never forget the sensations I felt, on awakening one morning, while residing at Algiers, I started out of bed almost suffocated, and on rushing to the window the sensation was increased—the air felt like the steam issuing from a large boiler.—

I felt oppressed, and panted for breath—nothing gave me relief until the sea breeze set in, when I felt delivered from the most overwhelming load, and enjoyed the transition with the most pleasing delight. I can compare my feelings only to nightmare, or partial drowning.

Hippocrates says, that excessive heat effeminates the flesh—debilitates the nerves, and stupifies the spirits.

Europeans, says Lord Kaimes, when they arrive in the torrid regions of America, lose their vigour and colour in a few months: their motion is languid, and their words are pronounced in a low voice, and with long and frequent intervals.

Caloric exerts a similar power on the vegetable kingdom. Plants indigenous to a cold climate, when changed to a warmer one, have their vital principle overcome by the heat, grow languid, wither, and die. The blight which is so destructive to grain, originates frequently in a sort of sultry and pestilential vapour. It was this, says Hale, which used to damage the vineyards of ancient Italy. The Romans observed that it generally happened after short showers occurring about noon, followed by clear sunshine, about the season of the ripening of the grapes, and that the middle of the vineyard suffered most. It is this which damages our wheat crops and hop plantations.

Dr Black concludes his observations on heat, by stating, that, "were the heat which at present cherishes and enlivens this globe, allowed to increase beyond the bounds at present prescribed to it; besides the destruction of all animal and vegetable life, which would be the immediate and inevitable consequence, the water would lose its present form, and assume that of an elastic vapour like air; the solid parts of the globe would be melted and confounded together, or mixed with the air and water, in smoke and vapour, and naturewould return to the original chaos."

We thus observe, that, in a moderate temperature, man is stimulated to exertion, but that whenever the temperature is increased in warmth beyond what his body has been habituated to, he languishes and dies.

We are entitled, then, to conclude, that a high degree of caloric is a powerful sedative to animated nature.

I shall now say a few words on the manner of its operating on the healthy animal body, and then apply these to inflammation.

The effects of caloric on the human system have been hitherto considered stimulant. This seems to have arisen from not reflecting that every stimulus, when increased, becomes sedative, and chiefly from viewing caloric as the stimulus, and cold as the sedative. We observe that the mild rays of the sun animate the living principle both of the animal and vegetable kingdom, but when increased beyond this enlivening effect, that they overpower and ultimately annihilate it. The same is observable of cold. To deny that a moderate degree of caloric is stimulative, would be absurd; equally so is it to deny, that a great degree is sedative. Every stimulus possesses the same powers, and produces similar results A moderate quantity of wine enlivens and cherishes life—a greater quantity excites it too much,—while an enormous quantity destroys life. Opium operates in a similar manner.

Caloric, in a moderate degree, enlivens us, and renders us cheerful, by rarifying the air, so as to enable us to breathe with greater ease; by raising the temperature of the ambient atmosphere, which warms and gives the skin a pleasant feeling; and perhaps by charging our systems with the electric fluid. These effects seem to be produced by the caloric impressing first the nerves, which afterwards influence the other organs.

A greater degree of caloric oppresses the nervous system, causing languor, sickness, and fainting, as well as profuse perspiration. Were this degree of caloric continued, the powers of life would be subdued. And when a very high degree of caloric is at once applied, it destroys the whole animal fabric.

The perspiration seems essentially requisite to our existence in a warm temperature; it moderates its influence by carrying off the latent caloric in a similar manner to the vapour of boiling water. The experiments of Fordyce and Blagden prove, that the warmer the surrounding medium the greater is the evaporation; and the more the air is saturated with moisture, the greater effect it has. A degree of heat at 211° did not at all affect their respiration, but shook their hands, and caused a considerable degree of languor and debility. Sir Charles Blagden, on touching his side, during one of the experiments, says, that "it felt cold like a corpse." The increase of the pulse did no harm, and I should conceive, that wherever perspiration was established, this would be as harmless as quickened circulation from exercise.

Dr Franklin describes the effect of a sultry day at Philadelphia as follows, (the thermometer was 100° in the shade.) "The sweat ran off the backs of my hands, and my shirt was often so wet as to induce me to call for dry ones to put on. My body," continues he, "never grew so hot as the air, or the inanimate bodies immersed in the air."

The late ingenious Dr Currie, although prepossessed in favour of cold bath in fever, thus expresses himself on the warm. "Experience convinced me, that the practice of the tepid affusion, or water heated from 87° to 97°, in many cases lowered the heat of the living body as speedily as when the affusion of cold water was used." "In some cases, if I mistake not, the heat is lowered more speedily by the tepid water. The evaporation from the surface is more copious from the tepid affusion, and on this the cooling of the body very much depends. But this is not all," continues he; "the tepid affusion is little, if at all stimulating, and does not, like the cold affusion, rouse the system to those actions by which heat is evolved, and the effects of external cold are resisted. When the object is to diminish heat, that may be obtained with great certainty by the repeated use of the tepid affusion suffering the surface of the body to be exposed in the interval to the external air. And if the beams of the sun are excluded, and a stream of wind blows over it, the heat may thus be reduced, where cold water cannot be procured, even in the warmest regions of the earth, in the plains of Bengal or the sands of Arabia." The exposure of the body to the cold air after the affusion is not at all necessary, and as it is liable to be followed by serious consequences, it should never be had recourse to.

In another part of his Medical Reports, he takes notice of the salutary effects of tepid bathing after exertion in the West Indies. "It prevents," says he, "the dangerous reaction of the centre, which a torpor of the extremities so generally produces. It allays the violent action of the heart and arteries, and soothes the system of sensation." He found that the hot bath did not raise the heat of the body as was expected, although previous immersion in the cold had been practised. "Hot baths," says Hippocrates, "extenuate and cool those who use them; for by the heat, they evacuate the serum of the body, and that being evacuated, the body is cooled."

Almost all medical writers of any experience recommend hot applications to one species or other of acute inflammation, but no work am I acquainted with, where it is laid down as an invariable principle to use these remedies in this disease. Even J. Hunter, who insists so much on cold, says, that in irritable inflammations, warm fomentations are had recourse to, and that, if the pain arises from the contraction of the inflamed vessels, they do good. Now, wherein can consist the difference of acute inflammations, but in their degree?

For ten years I have used invariably hot anodyne applications[*] to every acute inflammatory disease, and have never found them fail, in either mitigating or arresting the disease. I have used them in erysipelas of the face and leg, in acute ophthalmia, in acute cynanche tonsillaris and trachealis; in pneumonia, in enteritis, in acute gonorrhœa, &c. with wonderful success. Care must be taken in those inflammations attacking serous and mucous membranes, that, when the activity of the inflammation has been subdued, the treatment must be instantly changed to an invigorating plan.[†] This is well exemplified in ophthalmia and gonorrhœa, where, if the heat be too long persevered in, the ophthalmia becomes chronic, and the gonorrhœa, gleet. This I have particularly explained in a little probationary essay which I wrote, when I became a Fellow of the Royal College of Surgeons of Edinburgh. In that essay I try to establish the principle of using hot anodyne applications in acute inflammation. Nothing can be more inconsistent than injecting into an inflamed urethra, a caustic solution of the nitrate of silver. It is, I apprehend, from the want of a rational theory of disease, that such inconsistent methods of practice are daily proposed.

I have used with equal success the warm bath in the commencement of all kinds of fever, in yellow fever, typhus, synocha, synochus, scarlatina, measles, &c. for I consider every fever in the beginning to be inflammatory, and to be more or less so, according to the less or greater affection of the nervous system. The warm bath would be more efficacious, did it contain opium in solution.[‡]

I trust, from the facts which have been brought forward, it appears evident, that caloric is a powerful sedative; that it is very effectual in subduing acute inflammation; and that it is more at command and more manageable than cold.

"The third means of practice," says Lord Bacon, "regards that grand engine or instrument both of nature and art, viz. heat and cold. And here the human power seems perfectly lame on one side; for we have the heat of fire infinitely stronger or more intense, than that of the sun as it naturally comes to us, or than the heat of animals; but cold is wanting unless so far as it may be procured by the severity of winter, by caverns, or by the application of snow and ice."

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[*] The manner in which I have employed them is as follows. A fomentation is made, by boiling two ounces of poppy-heads with two drachms of gum opium, in three English pints of water. With this the inflamed parts are fomented for an hour or more, twice or thrice a day, as hot as the patient can bear, applying during the intervals of fomenting, large hot poultices of onions, potatoes, turnips, barley-meal or oatmeal, every two hours; or oftener, should the patient complain of the poultices cooling; for, whenever this occurs, the inflamed part becomes painful.

[†] The restoring a part, or the whole system to its wonted vigour of health, should be invariably attended to after every disease.

[‡] At a future period I shall take an opportunity of making some further observations on fevers, and their treatment.

We shall now proceed to investigate the effects of cold.

The consideration of cold as the absence of caloric, induces us to view it as the removal of a stimulus, caloric only being supposed the positive stimulating principle. Consequently, the application of cold to the living body is presumed to abstract this principle, and when applied to it in a state of inflammation, to moderate the heat. We thus observe, that the viewing cold in this light, deceives us in its effects on the living body, for we shall find that it is a powerful stimulus for many degrees below the temperature of the animal, and that only at a very low degree it acts as a sedative. In the practice of medicine, heat has been considered, that range of temperature from cold water indefinitely specified to boiling water; while cold, the temperature of cold water or air depending on the season of the year.

We mentioned that 62° was the mean temperature on the surface of the earth, and that in the latitudes where this temperature prevails, man has attained the greatest excellence of body and mind. Now, a removal to the northward of this latitude for some distance, calls forth the vigour both of the body and the mind; and not until the temperature becomes considerably reduced, are the faculties impaired. Those inhabiting the northern parts of the middle latitudes are roused by the stimulus of the cold to more than ordinary exertion; they have greater firmness of muscular fibre, and possess greater strength. But when we advance beyond these, and enter the Arctic regions, the cold benumbs the faculties, and renders its inhabitants dull and slow, moderate in their desires, regular and peaceable in their manner of life. It is only the most intense cold that produces sleep and death.

"By excessive cold," says Dr Black, "animals are seized with sleep and insensibility. And in those countries where the colds of winter are violent, the inhabitants are on their guard to shake off the first appearance of drowsiness." These effects of cold are corroborated by several authors;—by Quintus Curtius in his account of the army of Alexander the Great while in Scythia; by Outhier, as well as by Hellant, in their description of the Laplanders; by Gmelin's account of Siberia; and by Sir J. Banks's interesting Narrative when he landed in Terra del Fuego. The fact of Richmond, one of the party, who was roused from his lethargic state, in which he had indulged for some hours, while three others awoke no more, shews that cold oppresses the nervous system for some time before it entirely destroys it, and produces congelation. This equally proves that intense cold is requisite to subdue the nervous energy; for, Sir Joseph, Dr Solander, and the others, resisted its effects for some time before it overpowered them. In the northern latitudes we have equally, as in the southern, immense tracts of country deserted by animal and vegetable life.

Plants habituated to a warm atmosphere, when exposed to a climate of a greater degree of cold, suffer a corresponding diminution in the force of the circulation of their fluids, and in the quantity of matter transpired. Every function of the plant becomes more torpid. It gradually withers and decays.

We thus observe that either of the extremes of heat or cold have the same effect on animal and vegetable life; but, that a moderate degree of each produces a different result.

Cold, at any degree, applied for a short while to the animal body first excites the nerves, which in their turn raise the action of the arteries, veins, and lymphatics. But when long persevered in at an intense degree, cold oppresses the nerves so as to induce sleep and torpidity; then breaks the vital principle and causes congelation.

Hippocrates says, that excessive cold blackens inflammations by contracting the skin, and that it hinders perspiration and the circulation of the blood through tumours. He also states, that if any person in health cools himself very much in the winter time, either by bathing in cold water or otherwise, the more he is cooled, provided his body be not actually congealed, the more violently will he become hot when he dresses himself and goes into a house. This effect is also substantiated by the natives of Finland and Muscovy plunging into the snow to excite warmth in their bodies.

The stimulating and invigorating effects of cold-bathing seem to have been known in the earliest ages. Homer mentions that the Greeks practised cold immersion both for purification and to strengthen the animal faculties.

A gangrene, says Wiseman, doth arise in phlegmons through the unseasonable application of over cold medicaments.

Boerhaave states, that he has known men of Herculean strength flung into incurable rheumatism from the cold of one night, by sleeping on the wet grass. Pneumonia is often caused in a similar manner, as in cases of shipwreck. Chilblains are also similarly induced. Inflammations are more frequent in cold climates, and during winter, than in warm climates, and during summer. Boerhaave elsewhere asserts, that the first of mankind were not sensible, till they had experienced, that, by the taking of cold aliments into their bodies while very hot, there was danger of causing a present pleurisy and death.

Heister relates a case of a young man who, when heated, took a draught of ice cold water, and that immediately inflammation of the stomach followed, of which he died. It is well known that this viscus, when inflamed, rejects cold water sooner than any other beverage, and that it produces severe pain and intolerable heat.

Rush says, that persons who have recovered from the immediate danger of drinking cold water, are sometimes affected after it with inflammations and obstructions of the breast and liver.

Sydenham thus expresses himself on the effects of cold: "Sane existimo plures modo jam designato, quam peste, gladio, atque fame simul perire." Monsieur de Beauplan, in his description of the Ukrane in Poland, states, that the cold sometimes produces violent inflammation of the bowels, terminating in gangrene. On dissection, he says, that the intestines were found adhered together, black and gangrenous.

The elder Cullen talks of the diathesis phlogistica being often manifestly induced by the tonic or astringent powers of cold.

Darwin says, after immersion in cold water or in cold air, the whole system becomes more excitable by the natural degree of stimulus, as appears from the subsequent glow on the skin of people otherwise pale. Frequent cold immersions harden or invigorate the constitutions. "But if the diminution of external heat," continues he, "be too great, produced too hastily, or continued too long, the torpor of the system either becomes so great, that the animal ceases to live; or so great an energy of motion or orgasm of the vessels succeeds, as to produce fever or inflammation."

J. Hunter delivers his opinions on the operation of cold as follows: "Cold, according to its degrees, produces two very different effects; one is, the exciting of action without lessening the powers; the other is absolutely debilitating, while at the same time it excites action, if carried too far. In the first, it becomes like suitable exercise to the vascular system, as bodily exercise is to the muscles, increasing strength; but when carried or continued beyond this point, it lessens the powers and becomes a weakener, calling up the action of resistance after the powers are lessened."

The younger Cullen makes the following observations on cold. When mentioning the capability of the human body to resist the effects of cold, he says, "Et ita evenit, quod vis frigoris absoluta, corporis vivi temperiem nihil mutet, donec frigus adhibitum inferioris gradus fuerit, quam ille cui corpus vivum resistere valeat." In stating that it is scarcely possible to tell the degree that cold produces a change on the temperature of the living body, he thus expresses himself: " Sunt tamen adeo multa, quorum auxilio temperies corporis vivi, quamvis frigori violento expositum fuerit, sustinetur, ut quantam vim frigoris corpus vivum, auxiliis omnibus destitutum, pati possit, dictu foret difficile." After stating various reasons to shew that cold applied for a short time to the body acts as a stimulant, he states, " Videtur autem vim stimulantem manifesto locum habere in omni frigoris gradu a sexagesimo secundo ad trigesimum secundum usque, et etiam in multo inferioribus donec vis frigoris tanta sit quanta vim nervosam statim prorsus delere valeat." When mentioning the relative effects of cold, he says, " Hic sensus fere semper, partibus in quibus oritur stimulo est. Hæ enim partes statim fiunt rubicundiores, quam antea, et plerumque sensu frigoris evanescente, sensus caloris mox ibidem oritur." " Hinc etiam verisimiliter a vi frigoris tonica pendere videtur, quod homines, hyeme et in regionibus frigidis, quam æstate et in regionibus calidis, robustiores sint. Hinc intelligimus id quod sæpe observatum fuit, quare frigus sæpe parti singulari, præcipue autem toti corporis superficiei applicatum, tonum et robur per totum corpus augere valeat."

Dr Currie was of opinion, that cold acted as a stimulant. Throughout his work he talks of it as a stimulant. " Cold acting powerfully on the sensations," says he, " will in the first instance stimulate the system; as it appears probable, that, in proportion to this stimulation, is the difficulty of reducing the actual temperature. Hence it may happen that the *injudicious use* of *cold* in *inflammatory diseases*, shall *increase* the *action* of the *heart* and *arteries* by the stimulus of sensation, without subtracting permanently the stimulus of inordinate heat." He found that cold was also injurious in those fevers accompanied with local inflammation: now, scarcely a fever occurs, without causing more or less inflammatory determination to some of the principal organs; hence some have made fever to centre in the brain; others, in the heart and lungs; while others again, in the biliary system. He likewise observed, that the cold affusion was hurtful when the heat of the blood was below or even equal to the natural heat. It was only in those fevers where the heat was considerably increased, no local inflammation present, and the stamina of life sufficient to bear such a shock, that the cold bath was found serviceable. " In fevers," says he, " where the lungs are oppressed and the respiration laborious, this oppression might be dangerously augmented by the sudden stimulus of the cold affusion."

Quintus Curtius relates the risk that Alexander the Great ran, from plunging, while warm, into the cool Cydnus. When Alexander's army arrived at the Oxus, "they who drank immoderately immediately died, and Alexander lost more men this way, than he had ever lost in any battle."

Franklin relates the story of three young men falling a sacrifice to bathing in cold spring water, while heated with reaping.

Sir Everard Home performed the following experiments, which at once decide the superiority of warm applications over cold in inflammation. "The wrist of one arm was surrounded by bladders filled with ice, and after having remained in that state five minutes, the pulse was felt at the same time in that and the opposite wrist, and the beats were found evidently strongest in the cooled wrist. This experiment was then made with water heated to 120° or 130°, beyond which the heat could not be submitted to, and the pulse was found softer and weaker than that in the other arm." "When one wrist was cooled, and the other heated, the stroke of the pulse in the cooled arm had great force beyond that in the heated one."

I have not under cold brought forward the opposite opinions of the celebrated Brown; conceiving that the facts which are here stated must convince any unprejudiced observer. These facts may appear too numerous to some, but the prejudice which adheres to us from early education can only be overcome by multiplied authorities; and probably not even by these when we daily witness men high in their profession supporting this prejudice. Cold applications are daily employed in active inflammation; and cold bathing in inflammatory fever, by professional men of the first eminence.

It now only remains to say a few words on Narcotics. These contribute greatly to subdue the inflammatory action. We have already stated that opium should be dissolved in the external fomentation. It ought also, whenever its administration is admissible, to be given internally in the state of gum, but never in tincture. When its exhibition is inadmissible, henbane, hop, or foxglove, should be substituted. A warm bath having opium dissolved in it, I am persuaded, would have great efficacy in subduing inflammatory fever.

We have now detailed the remedies put in practice to subdue the pain; and as this symptom is the first and chief one, and arises from morbidly excited nerves, which influence the other vessels concerned in inflammation, it naturally follows, that the other symptoms will be benefited by the treatment which has been already used. The *heat* which we enumerated as the second phenomenon, will be reduced, first, by the bleeding; secondly, by the hot anodyne fomentations; and thirdly, by the narcotics, for we have proved that heat is generated by nervous excitement. When the inflammatory action is severe, or has affected the general system, or when an important viscus is the subject of disease; cooling purgatives must be administered, and the patient put on low diet and diluent drinks.

The *redness*, or the third symptom, being consequent on increased arterial action, will likewise be rendered paler by the reduction of nervous excitement, for we have proved that arterial action depends on nervous influence. The arteries therefore will circulate their fluids more languidly. But as they have been overdistended by the inflammatory action, and by the subsequent relaxation, the redness will not disappear in a ratio to the pain and heat, as is well exemplified in chronic ophthalmia. It is hence requisite, after the acute inflammatory action has been subdued, to use restorative means to recruit the debilitated part to its wonted vigour of health. This is best accomplished by giving up the warm relaxing applications, and using water colder and colder, but always applied for a short while at a time, having recourse to friction when the part will admit of it, and using a roller when it can be applied. Attention should also be paid to diet and exercise if required.

The last phenomenon of inflammation mentioned was *swelling*. This occurs in a manifest degree only when the inflammation has been severe and of some continuance. The remedies which have been used will both reduce and increase it; the bleeding, judiciously performed, will lessen it, while the warm relaxing applications, by favouring the effusion of coagulable lymph, will augment it. This symptom therefore will be removed more by the restorative than by the debilitating means. It is generally a phenomenon of less consequence than the others, and its magnitude depends entirely on the violence of the others.

I have not illustrated this treatment with cases, conceiving that to bring forward the opinions of such celebrated men, all corroborating it, will have much greater influence on the profession. As many, however, may suppose it is more speculative than practical, I shall detail several cases in a future number.

Edinburgh, 6, *George Street*, *March* 1, 1819.

IX.

On the Effects of the Solution of Arsenic. By JOHN ASTBURY, M. D.

MR —— aged 51, consulted me on account of a violent pain, attended with swelling, on the left parietal bone. The glands on the right side of his neck were in a state of ulceration, and there were other signs of scrofula in his habit. Blisters, and occasionally leeches, with cold applications of salt and water, afforded temporary relief, but the pain in the head frequently returned with violence. He had had no venereal complaint for ten years; his appetite and general health, in other respects, were good. He then applied to another medical gentleman, who directed mercury to be used until the mouth was affected. For five weeks he found little or no relief from this plan. He then tried the solution of arsenic, in the dose of ten drops twice a-day, which frequently made him sick, and excited vomiting, and made him feel uneasy and unwell all over him. In the space of a fortnight after he began to take the solution of arsenic, his legs began to be painful, and swell, towards evening, so as to hang over his shoes. The dose of the solution of arsenic was then diminished to seven drops twice a-day. This dose agreed better; his legs, though they continued to swell in an evening, were not so painful, and he was free from sickness. The dose was then increased to ten drops twice a-day, and continued for seven weeks. His legs continued to swell in an evening during that period; the pain in the head, though somewhat relieved, still continued. He then applied to me again. I directed some pills with gum guaiac, and to drink decoct. sarsaparill. several times in the day; to go to the sea, and use the warm sea bath; to live on animal food, with potatoes and milk, and drink three or four glasses of wine every day after dinner. In three weeks after taking the guaiacum pills, in doses of ten grains, and the decoct. sarsaparill. the pain of the head entirely left him; the swellings of the legs, for the first fortnight he was by the sea side, were relieved; they then began to swell again every night, and continued to swell for five weeks. When he left the sea, he called upon me on his return, and said that he was perfectly well, except the swellings in his legs. His appetite was very good, he slept well, his general appearance was healthy, and he could walk six miles with ease. I advised him to drink half a pint of port wine after dinner; to continue his diet of milk and animal food; to try the effects of his native air; and, if he did not go on well, to call on me again. This was on December 22, 1817. On December 29th, he called upon me. His legs and thighs were very much swollen; the penis was anasarcous, and very much enlarged; the scrotum was likewise considerably enlarged; the lower part of the abdomen was hard and œdematous; his pulse intermitted, and his breathing was beginning to be difficult. He walked that day six miles to consult me, and did not experience any fatigue or inconvenience, except the difficulty in breathing. The penis and scrotum were suspended; his appetite continued very good; he had no thirst; he slept well, raised up in bed; his bowels were regular, and he voided a sufficient quantity of natural coloured urine. I directed some pills with gum guaiac and a grain and half of pulv. scillæ to be taken three times a day, with decoct. cort. cinchon. ℥iss. vin. ferri ʒi. I requested him to eat plenty of horse radish root, with his roast beef and mutton, and take a dessert spoonful of mustard seed every day, and drink half a pint of port wine after his dinner. He continued these medicines and regimen for a month with considerable relief. I then joined the decoct. senegæ to the decoct. cort. cinchon. and directed a grain of pulv. fol. digital. to each dose of the pills. These acted as a strong diuretic, and seemed to empty him too fast. He became pale and feeble. I directed the doses to be diminished, and only to take them twice a-day. He continued these medicines and his regimen for two months, and called upon me on March 30th quite well in health, except a trifling cold, and free from his dropsical complaint. The pulse in general was about 80.

When we reflect, that the legs began to swell and be painful in a fortnight after taking the solution of arsenic; that it excited vomiting, and made him feel uneasy all over him; that his general health was good; and the muscular strength not diminished; that the kidneys secreted a proper quantity of natural coloured urine, when in common cases of dropsy there is always thirst, the secretion of urine is diminished, and the urine high coloured, and the muscular strength depressed, I think there can be no doubt, but the swellings in the legs, and other dropsical symptoms, were the poisonous effects of the solution of arsenic upon the absorbents. My patient took the solution of arsenic until he went to the sea. The stimulus of sea air, with warm sea bathing twice a-week, seems to have sufficiently excited the action of the absorbents (during his stay there) in every part of his body except his legs, but as soon as that stimulus was removed, symptoms of general dropsy came on in the most rapid manner. The solution of arsenic is an useful medicine, when not continued too long, and often relieves periodical complaints; but the long continued use of it in this case produced a very dangerous torpor in the action of the absorbents, which threatened my patient's life; and the absorbents did not recover their healthy and proper action for near nineteen weeks after my patient had ceased taking the solution, and then only by the assistance of strong stimulants and tonics.

Barlaston, April 14, 1818.

X.

Case of Rupture of the Ligaments of the Atlas occupying the left Side of the Processus Dentatus, viz. the left Moderator, and left Side of the Transverse Ligament. By W. Howison, M. D. Extraordinary Member of the Royal Medical Society, &c.

I have taken the liberty of drawing up for publication the following history and dissection of a patient who died in the Edinburgh Royal Infirmary, from a rupture of the ligaments occupying the left side of the atlas and processus dentatus, permitting pressure to be made upon the upper part of the spinal marrow, when the head was bent forwards, and in that way proving fatal to the individual.

My reasons for so doing are as follows: That, in the above case, which appeared to me to be a very unusual and uncommon one, the dissection was carefully and accurately performed,—the morbid appearances detected accounted well for the symptoms and death of the patient,—that, to the best of my very limited knowledge, there were very few, or perhaps no case, of a similar nature on record,—and, lastly, and principally, that it afforded me employment and amusement in my vacant hours. The following description of the above case is very far indeed from being so accurate and comprehensive as it might have been, as, at the time when the patient was alive, and in the hospital, I had not the slightest nor most distant intention of making it public; but, upon later consideration, the idea that it might be of some consequence to medical science in general, whereas it could do no harm to it, and having spare time on hand, I resolved to attempt to make it as minute and accurate as circumstances would allow, however imperfect and deficient it might turn out to be when completed.

A. B., the subject of this interesting and unusual case, was admitted as a patient into the Edinburgh Royal Infirmary on the 2d of August 1816. He was a middle-sized delicate-looking man, of a spare habit of body, sallow complexion, and 48 years of age. He had served as a private soldier in the army for a number of years, during which period he had been employed in very active service in different climates, and had undergone a great deal of fatigue. After having been discharged, he supported himself and family by acting as a street porter, in which capacity he was in the habit of carrying heavy loads, and particularly, from custom, as he said, over his left shoulder.

He reports, that, after having enjoyed good health for a number of years, he was attacked for the first time, nearly seven weeks ago, with a dull sort of pain, affecting his occiput, and back part of his neck, and that of late it has become gradually worse, and much more constant. About a week ago, he was attacked with pain in the anterior part of his throat, occupying the course of the trachea and œsophagus, and accompanied with considerable difficulty of deglutition, which has gradually become so bad that now he is rendered entirely unable to swallow almost any thing.

During the course of these last three days, he has had three different blisters applied to the region of his neck, which run well, but without procuring him any, or at least very little benefit. He has also had repeated laxatives, with the effect of keeping his bowels gently open.

At present, he complains of the sensation of a constant, dull sort of pain affecting his whole head in general, but particularly his forehead and occiput; from that it stretches down, attacking both the posterior and anterior parts of his neck, but at present it is principally confined to the fore part of it, affecting the course of the trachea and œsophagus. He is unable to move his neck, or upper part of the trunk of his body, and when food is given to him, he is obliged to be supported in the semi-erect posture in bed, with his head inclined backwards. When fluids are put into his mouth, and he attempts to swallow them, they appear to be received a short way into the œsophagus, and are instantly rejected, without any, or very little of them, reaching his stomach. When answering questions which are put to him, he articulates very slowly, and it appears to give him pain and uneasiness. For some time past, he has been under the necessity of keeping himself in the semi-erect position, with his head inclined backwards; and when he goes to sleep, he is obliged to be supported in bed with pillows in the above posture. He sleeps but little, and what he does procure is generally disturbed, interrupted, and is followed by little or no refreshment. His breathing at times also appears to be considerably affected and difficult; and, from the very visible flushing and turgescence of his face at times, there appears to be a considerable determination of blood to his head. His pulse is about 60, full, but irregular. His tongue is slightly foul and parched, and he is troubled at times with urgent thirst. His bowels, he reports, are naturally costive. His skin is hot, and bedewed with perspiration. He has rigors at times, succeeded by heat.

After his admission into the Edinburgh Royal Infirmary, under the care of Dr Hamilton, senior physician to the house, the following reports were made by that very accurate, and justly eminent man.

August 3d.—Applicent. hirudines vi. part. cervicis qua dolet. Injiciatur per anum juscul. bovin. ℥viii. 3tia vel 4ta. q. q. hora.

Inhalet vapores aq. fervent. inter respirandum.

4th.—Pains continue. A little beef-tea, however, has been swallowed. The injections have been retained. Paralysis of the left side, but particularly of the superior extremity, has now supervened. Leeches succeeded rather ill.

Oblinat. cervix tam posterior quam anterior. ol. ammoniat. Contin. enemata et juscul. bovin.

5th.—Paralysis of the whole right of the body has likewise occurred. Pains of the hind neck and of the superior part of the cervix, still impeding deglutition, continue. Respiration has become laborious and frequent. Pulse feeble.

Contin. Ol. ammoniat. et juscul. bovin. per anum injiciant. To have a night nurse.

7th.—Accipiat enem. domest. statim.

8th.—No alvine evacuation. Respiration is less difficult; and, by report, a considerable quantity of fluid has been swallowed. He is now in the recumbent posture, and asleep.

Habeat. enema purgans. Juscul. bovin. ut supra præscript. Ova absorbend.

9th.—Beef-tea has been taken; and, towards evening, a portion of sac whey ordered, was returned. He continues in the recumbent posture, and has enjoyed some sleep. Pulse is regular, but feeble. Tongue clean. Alvine evacuation. Countenance appears somewhat flushed. He complains of slight headach, and of complete loss of the power of moving any of the extremities. About lb. iii. of urine passed through the catheter this morning.

Habeat serum vinosum cum vini rubri ℥iv. confect. Cont. juscul. bovin. Applicentur hirudines vi. fronti vel temporibus.

10th.--Headach continues, with fulness of features, and a florid flushing of countenance. Paralysis as described. Respiration laborious.

Habeat vin rubri ℥viii. Cont. alia.

11th.—Died yesterday, shortly after the visit.

As in this very interesting and unusual case it became of great consequence to discover what could have produced the morbid symptoms above described, permission was requested to open the body, which was immediately granted, and was performed two days afterwards in presence of several of the medical gentlemen of the hospital.

Dissection.

Upon dissecting back the integuments of the head in the usual manner, and removing the skull cap, the superficial veins running along the surface of the pia mater and brain, and also the various sinuses, particularly the superficial longitudinal and lateral ones, were observed to be considerably full and turgid with venous blood. Upon cutting down to the ventricles, and laying them open throughout their cavities, they were found entirely sound and healthy, one of them only appearing to contain a very small quantity of serous fluid. All the other parts of the cerebrum and cerebellum were found healthy, excepting that there was a considerable turgescence of blood in the deeper seated vessels.

The body being laid upon its abdomen, an incision was made from the occiput along the spinous processes of the superior cervical vertebræ; and the integument and muscles occupying the back part of the neck were dissected back, so as to expose the upper part of the spine; which being done, the second bone of the neck was found to be much more moveable than in its natural state. On removing the cerebellum from the skull, and attempting to thrust the finger into the spinal canal through the foramen magnum of the occipital bone, when the head was bent forwards, great resistance was given to it. On moving the head backwards, however, the resistance was removed, and the finger passed easily into the vertebral canal; but on again bringing the head forwards when the finger was in the abovementioned position, it was very forcibly squeezed, and forced out from its situation betwixt the back part of the foramen magnum, and what appeared to be the processus dentatus of the second cervical vertebræ. The atlas and the second of the cervical vertebræ being carefully dissected out from the spinal canal and examined, it was found that the ligaments of the left side of the processus dentatus (viz. the left side of the transverse and the left moderator ligament) were ruptured, and allowed the processus dentatus to project backwards at the time that the head was bent forwards, so as to press upon, and, of course, destroy the functions of the spinal marrow ; and, consequently, that this produced the paralysis, &c. and caused the patient's death.

Upon examining the trachea and œsophagus, they were both found to be in their healthy and natural condition, excepting, perhaps, a very slight appearance of inflammation being present.

Edinburgh, *Royal Infirmary*,
April 1, 1817.

PART II.

CRITICAL ANALYSIS.

I.

Medico-Chirurgical Transactions. Published by the Medical and Chirurgical Society of London. Volume IX. (in two Parts.) 8vo, pp. 500.

Our praise of the Transactions of this Society has hitherto been almost unqualified, and the council of the society alone are responsible, if they should cease to merit it. The abundance, we may almost say the excess, of materials which must be presented for publication, will require very great vigilance and independence in their selection; and indifferent communications should be rejected, though the productions of authors of acknowledged celebrity, or recommended by the highest patronage. This volume contains, in the whole, thirty papers; twelve of which are surgical, nine medical, four pathological and physiological, and five miscellaneous. Our abstract, therefore, must necessarily be very short.

The first paper connected with surgery is, a *History of a Case of Cæsarian operation, in which the Lives of the Mother and the Child were both preserved*; by Dr Locher of Zurich. The pelvis was in this instance so much deformed by rachitis, as to prevent the introduction of the forceps and to render dangerous any attempt at destroying the child, and bringing it away by portions at a time, and there was every reason to believe that the child was still living. The Cæsarian section was therefore resolved upon. For a minute description of the operation, we must refer to the original paper; it will suffice to say, that the incision was made upon the linea alba, and that, in the performance of it, but few difficulties occurred, the intestines being easily prevented from protruding at the wound, and the hæmorrhage was little. The uterus being opened by an incision of six or eight inches in length, the child was slowly removed, the head being last disengaged; it was living, and soon cried vigorously. On detaching the placenta, there was a copious hæmorrhage, which was sponged out, and the cut edges of the uterus were brought together; after which the wound in the integuments was closed by five sutures. The abdomen soon became hard and distended, but not painful on pressure, and in three hours there was an hæmorrhage by the vagina. The lochia appeared in due course; and, until the evening of the fourth day, no unfavourable symptom had occurred, when, after having fatigued herself by attention to her child, and talking to visitors, the patient was suddenly attacked with convulsive spasms; these, however, were soon subdued, and she continued to do well. In seven weeks menstruation recurred; and in two months the woman was perfectly well. We think this case in the highest degree interesting and important; it affords much encouragement to practise this operation. The prospect of success, however, must be greatly influenced by its being adopted early; and the dreadful circumstances which can alone justify the Cæsarean section, are such as may generally be pronounced upon at the commencement of the labour.

Dr Albers gives a *case of Inguinal Aneurism, cured after the use of compression.* The patient having refused his consent to the operation of tying the artery, the aneurism, which appeared in the groin, was compressed by a pad, and the degree of pressure was regulated by a screw. After using the compression for two months, the violent pain and swelling which it occasioned in the limb rendered it necessary to discontinue it for a week, after which it was resumed for five months more, when all pulsation in the inguinal artery had ceased, and the aneurism was cured. The limb of the affected side was at first weaker, but the free use of it remained. The tumour at one time, during the use of the compression, increased in size, from an attack of inflammation; and, in a case which is mentioned at the end of this paper, where no such mark of inflammation appeared during the use of compression, though continued for six years, the aneurism was not cured.

Mr Kinder Wood of Oldham has proposed *a new method of operating for hydrocele, without procuring an obliteration of the tunica vaginalis.* His mode of cure consists in first puncturing the hydrocele with a broad lancet, and after discharging its contents, drawing forward the edge of the tunic, and with a pair of scissors removing a small portion of it. This process excites a mild degree of inflammation, which restores the tone of the exhalents, or absorbents, or both, but is not so great as to cause adhesion between the tunics. In the radical cure, by stimulating injections, there is an uncertainty as to the degree of inflammation excited in patients of bad constitution, or advanced in years; and this disadvantage will certainly render Mr Wood's mode of operating to be preferable, provided it shall prove generally successful, on more extensive trial.

Case of Aneurism in the Arm, cured by tying the subclavian artery, by Dr Post of New York.

Little difficulty appears to have occurred in performing this operation. The formidable circumstances in tying the subclavian artery, almost entirely depend on the aneurism having elevated or displaced the clavicle and other parts connected with the operation; none of which were present here. In Dr Post's case, livid spots had formed on the tumour, which sloughed nine days after the operation, leaving an ulcer, which did not prove of any importance.

There are two cases in this volume by Mr Wardrop. The first is that of a species of Nævus which the author proposes to call subcutaneous, and which he distinguishes from Mr Bell's aneurism by anastomosis, and from what this gentleman calls Cuticular Nævus. He describes the subcutaneous nævus as a more formidable disease than that last mentioned, being very dangerous to remove when of considerable size, on account of the number and size of the blood-vessels which compose it; and he mentions very candidly the death of a child from hæmorrhage, caused by cutting out a tumour of this description, although he had taken pains to cut beyond the limits of the tumour, and had removed it speedily. There is a drawing of this tumour, showing the vast size of its vessels. The patient whose case is the more immediate object of this paper was an infant, with a subcutaneous nævus, forming a large tumour on the left cheek, which had sloughed, and formed an ulcer about three inches in diameter, and which had so greatly reduced the child's strength, that it was necessary to have recourse to a generous diet before attempting the operation. The common carotid artery was tied, which much reduced the size of the tumour, and at first the health improved, but the ulcer soon spread over the whole diseased mass, and the patient died on the fourteenth day. The result of this case, however, is far from militating against the operation, and affords, we think, good ground to hope success, where it is practised before the general health has been destroyed by previous long suffering.

Mr Wardrop's second case is one of aneurism by anastomosis of the finger, which, after many unsuccessful means were tried, (amongst which was tying the two arteries of the finger,) was cured by an incision carried all round, through the integuments, avoiding only the flexor and extensor tendons.

Case of an Extensive Wound from the Bite of a Shark, by Dr Kennedy. The escape of the patient from immediate destruction was certainly miraculous, and his recovery from so extensive a wound more than could have been expected. But the detail is too minute, and occupies too much space in a volume where we look for instruction in every page.

Dr Vose of Liverpool has given a very valuable case of hydrocephalus, in which the water was removed by puncturing the head four several times. The patient was a child seven weeks old, in whom ossification had made but little progress, and the head was enlarged to between two and three times the natural dimensions, and had become transparent. In puncturing the brain, therefore, but little of the substance of that viscus could have been wounded. The first puncture was made with a couching needle, and three and a half ounces of limpid fluid were drawn off, but in a few hours about as much more had drained away from the wound, and the child became alarmingly exhausted; it, however, revived without the aid of medicine. After seventeen days a second puncture was made with less caution. a pointed bistoury being used for that purpose, and five ounces of fluid were evacuated without any unfavourable symptoms succeeding. The head, notwithstanding, regained its size, and was again punctured after twenty-two days, when eight ounces were taken away without any constitutional disturbance following. The last puncture was made in seven days more, and twelve ounces of fluid came away, leaving the head flaccid and shapeless, but no derangement of health succeeded.

> " A short time after the last operation the child was perceived to discharge a considerable quantity of water by the bowels. This at first took place with the natural motions; but afterwards the water, resembling in its sensible qualities that discharged from the head, was evacuated alone, and continued to be so for four or five days. The same low state as followed the first puncture of the head took place on the second day of this discharge from the bowels, and it was particularly remarked, that a diminution of the size of the head had corresponded with the quantity of water evacuated."

No medical treatment was observed except keeping open the bowels. Ossification recommenced, and continued to make considerable progress after each evacuation of fluid, until the bones of the cranium became as complete as is usual in infants.

There is a paper by Professor Maunoir of Geneva, recommending in various diseases the use of actual cautery, a long neglected remedy, which the Barons Larrey and Percy have of late done much to recall into use. Amongst other cases, M. Maunoir relates that of an English gentleman then at Geneva, who for fifteen years had an ulcer on his lower lip, which has resisted all the means tried by various surgeons in England and on the Continent. The edges of this ulcer, being cauterized, sloughed off, leaving a large sore, which had no longer the malignant character of the original ulcer, and which healed in three weeks.

Mr Travers has described two cases of aneurism, in which the temporary ligature was employed. The first was of the brachial artery, for which the ligature was allowed to remain for fifty hours before it was removed, when all pulsation had ceased. The second was a popliteal aneurism, and the ligature, which was applied on the usual part of the femoral artery, was removed after twenty-seven hours, but the pulsation returned in the artery. The final result of Mr Travers's experience upon this subject is, that he abandons altogether the use of the temporary ligature; and his reasons for this decision are, first, that, unless the ligature is suffered to remain for a considerable length of time, the artery does not become impervious; and, secondly, that the violence done in removing the ligature so much interrupts the process of union and granulation, that the wound is prevented from healing sooner than if the ligature were left to be thrown off in the usual manner.

A contribution *on Fungus hæmatodes* from Mr Langstaff, whose former cases of this disease appeared in the Society's eighth volume. The more we learn of this dreadful malady, the more reason we have to apprehend that all curative means are unavailing; and the present cases shake our reliance even on the last resources, amputation or excision; as the presence of fungus hæmatodes appears to be accompanied by a habit of body unfavourable to healthy restoration when an operation is resorted to. This disease does not appear to be regulated by any definite law in its attacks upon the human frame; in some cases affecting an individual limb or viscus; in others, invading every part of the body at the same time, or leaving one viscus only free from the disease.

We shall next proceed to the medical papers contained in the volume before us.

The first is a case of loss of power over the voluntary muscles, by Dr Bostock, in which, upon dissection, no appearance of disease was found in the brain or spinal chord, except a slight furrow on the latter, where it passes through the atlas;

that bone itself being rather thickened and yellow. The author asks, whether it is possible that a diseased state of the *muscles* may exist, which renders them insensible to all impressions from the nerves, although the latter retain their functions? But it certainly is not easy to conceive the existence of such a state of the muscles while they preserve a healthy appearance.

Dr Arnold of Stamford has given a case of Cynanche laryngea in an adult, which he treated with great judgment and with success, by the prompt application of thirteen leeches, free venesection, blisters, and calomel in doses of half a scruple every four hours.

Mr Martin of Pulborough relates a *case of Hereditary Ichthyosis*, in which, the mother having been affected with this disease from her youth, her daughter, then three years of age, was affected with it likewise, and in a much greater degree.

Dr Ferguson, who gave in the eighth volume of these Transactions a paper on the Origin and Progress of Yellow Fever, has in this one communicated the answers of Dr Musgrave to his inquiries on the same subject; the tendency of which is to prove the non-contagious nature of that malady, and the liability of persons once affected by it to subsequent attacks.

A case of mercurial erethism, of which the author was himself the subject, is related by Dr Bateman. The sufferings that attend this attack appear to have been extremely violent and alarming; and but for the author's own professional skill, and his great firmness in submitting to the measures necessary to relieve them, would probably have proved fatal. The whole effect of mercury, when it thus acts as a poison, appears to be exerted on the heart and circulating system. In Dr Bateman's case, the energy of the heart was so much reduced, that the sedative effect of sleep, added to the previous languor of circulation, occasioned symptoms which seemed to indicate immediate death; and for several nights it was found necessary to prevent the continuance of sleep beyond ten minutes, or a quarter of an hour. Dr Bateman continued to experience some irregularity of the circulation even at the time when he wrote this account, which was more than a year after the original attack.

Dr Badeley has communicated a case of general discoloration of the skin, after the internal use of nitrate of silver; and he mentions, that, on applying a blister, the new skin was white, as is found to be the case in persons of colour, after vesication.

The *Report of the Principal Diseases which occurred among the Gentlemen Cadets in the Royal Military Colleges at Great Marlow, Bucks, and Sandhurst, Berkshire*, occupies more than forty pages, the greater part of which is taken up in relating the general treatment adopted by Mr Bruce in *fever, intermittent fever, cynanche parotidea, scarlatina, variola, epilepsy, and convulsion.* Upon none of these subjects do we find any thing original or deserving of notice. The few pages of medical topography at the beginning, and the tabular view at the end, are all that deserved to be printed in a volume of this kind.

A case of defective power to distinguish Colours, by Dr Nichols of Ludlow, much resembles a former one, communicated by the same gentleman in the seventh volume of these Transactions.

Mr Rumsey has given three "*cases shewing the coincidence of worms in the intestines with hæmoptysis*," which, though not in themselves decisive as to the point in question, will fulfil the intention of the author, by directing the observation of members of the medical profession to the subject. In the two first cases, however, the hæmoptysis was accompanied by other and considerable derangement of the general health, besides the presence of worms; and in the last, which occurred in the West Indies, it appears, that the patient was in the habit of drinking spirits freely, which renders it probable that the hepatic functions and general health were injured.

We come next to one of the most interesting papers in the volume; *on the comparative unfrequency of Urinary Calculi among seafaring people*, by Mr C. Hutchison. "I have taken," says the author, "considerable pains to ascertain the prevalence of calculous disorders in the naval service, and the result of my inquiry is, that out of the mass of individuals of which it is composed, (and in another place Mr H. states the number of men serving to have been about 162,000 annually,) only eight cases have occurred in the period of sixteen years."

The space above mentioned includes a period of very active service, viz. from January 1, 1800, to December 31, 1815. And from documents furnished to the author, by the commissioners for conducting the sick and wounded department of the navy, he ascertains that the proportion of calculous cases to others, excluding of course gunshot wounds and accidents, is only one in 10,750 patients.

> "Of the eight calculous concretions before mentioned as having occurred out of the vast mass of patients admitted into our naval medical establishments in England, two were boys about 14 years of age, who had laboured under symptoms of stone for some years previously to their admission into the service, and into which they had recently entered, expressly for the purpose of deriving benefit from our magnificent institutions; one was a marine, about 22 years of age, who had been at sea a few months only; three were adult sea-

men, and the seventh a marine ; but their length of service could not be at all ascertained. The eighth and last case was a warrant officer, advanced in years, who had been serving in ordinary, that is, in a ship in harbour, for a considerable time previously to the operation, and the only case which terminated fatally."

We regret that the length of this article prevents our giving Mr Hutchison's reasoning on these extraordinary and valuable facts ; and we can only mention generally the evidence on which they are founded. This consists principally of the returns made from all the naval hospitals at home and abroad, and the replies to queries, addressed by the author to the professional persons who have the superintendence of them. The connection of the important inferences of Mr Hutchison with the pathology of calculous disorders is obvious; and, as we shall have occasion to remark in another part of our present number, they seem to disprove, or at least to modify in a great degree, the doctrine of M. Magendie, that animal food is the principal cause of the production of lithic acid in the human species.

Experiments on the Transfusion of Blood by the Syringe, by Dr Blundell. It appears, from the experiments of this gentleman, that the blood is not injured by being passed through a syringe, provided it be done speedily, but if drawn more than about 30 or 40 seconds before it is injected, it becomes unfit to support life. There is reason also to suppose, that the blood of one animal is not calculated to maintain life in another of a different species.

The very curious and important process *of the union of fractured bones*, where nature does so much, and the art so little, is very distinctly developed in a paper by Mr Howship.

Dr Prout has resumed his valuable *observations on the proximate principles of Urine.* His researches in this paper are extended to lithic acid, oxalic acid, and cystic oxide, and point out the relation of those principles, as well as of urea and sugar, with the albumen of the blood. Albumen obtained from the serum of diabetic blood yielded precisely the same components as that of healthy serum.

"To ascertain the relative proportions of azote, present in the diabetic urinous extract, and the extract from healthy urine, portions of them were burnt with the oxide of copper. The results were, that 100 parts of the diabetic extract, abstracting alkaline and earthy salts, yielded only 6.5 parts of azote, while the same quantity of extract from healthy urine yielded from 35 to 40 parts of the same principle."

"To obviate the objection that the saccharine matter of the urine might be formed, from the other principles of the blood, and not from the serum, comparative experiments were made with the cruor of healthy and diabetic blood. The results were precisely similar as to the quantity of azote in both, and, at the same time, the quantity of azote did not differ much from that found to exist in the serum above mentioned."—p. 476.

Dr Prout then gives the analysis of lithic and oxalic acids, cystic oxide, purpuric acid, and the pink sediment deposited from the urine of persons labouring under febrile and inflammatory diseases. In the last mentioned substance he found, in every instance, distinct traces of nitric acid.

The *expulsion* of a blighted fœtus and placenta at seven months, a living child remaining afterwards to the full period of utero-gestation, is related by Mr Chapman of Windsor.

Case of preternatural appearances in the ovarium, and female bladder, by Dr Phillips of Andover. In a cyst connected with the urethra, was found some hair and a tooth, fixed in a portion of bone resembling alveolar process. Extraordinary substances have been frequently in the ovaries and uterus, produced by diseased or unnatural action in those parts; but the most surprising feature in the present case we consider, is, that any process akin to that of generation should have taken place in a cavity not directly connected with the generative organs.

II.

Practical Observations on the Nature and Treatment of Marasmus, and of those Disorders allied to it, which may be strictly denominated Bilious. By JOSEPH AYRE, M. D. Member of the Royal Medical Society of Edinburgh; one of the Physicians to the General Infirmary at Hull; senior Physician to the Hull and Scullcoates Dispensary; and Physician in Ordinary to the Lying-in Charity at Hull. 8vo, pp. 256. London, 1818.

THIS book consists of three divisions. The 1st treats of the history and diagnosis of marasmus; the 2d of its pathology; and the 3d of its cure. At the close of the volume, there is, by way of appendix, a collection of cases illustrative of the whole. We shall briefly review each of these divisions in its order.

The history and diagnosis of marasmus occupy no less than ninety pages, and are pursued, we think, with too great minuteness. The author's facts and details seem to want that selectness and compression so essential to a spirited account of a disease. His descriptions are often tame—not to say tedious,—and arouse an irksome suspicion that they have been needlessly spun out,—a suspicion that is not wholly appeased even by the great value of the practical observations which here and there present themselves as we proceed. We willingly acquit Dr Ayre of the unfair arts of authorship—of any deliberate scheme to enhance the number or the cost of his pages by the procedure complained of: on the contrary, we are sure that he has erred from an honourable motive—an over-desire of being exact and true to nature. Nevertheless, we retain our opinion, that, by dwelling so long on minute features—by employing the same pains—the same *traits de plume* on the lesser or casual shades, as on the larger, bolder, and more constant features of the portrait, he has detracted considerably, if not from the faithfulness, at least from the strikingness of the general resemblance. This censure, however, is scarcely applicable, except to the first division of the work.

The disease here treated of under the name of marasmus, has received different appellations from different writers: it is the "worm fever" of popular phraseology; the "febris lenta infantum" of Hoffman; the "hectica infantilis" of Sauvage; and the "infantile remittent fever" of Dr Butter and other modern physicians. Dr Ayre considers it to be the same complaint as the bilious disorder of adults; but that marasmus and bilious disorders are really convertible terms, is an axiom that will not, we apprehend, be admitted by any one but a disciple of the hepatic school; unless, indeed, the term "bilious disorder" receive a greater latitude of interpretation than we are inclined to give to it. But of this more hereafter.

He divides marasmus into two species, the acute and chronic, —a distinction, we think, singularly judicious, and traced by the author with a much greater degree of clearness than heretofore. The acute readily passes into the chronic form, and *vice versa*: indeed, we believe the chronic form is almost always the precursor of the acute attack. In chronic marasmus, the patient is languid, drowsy, and morose; the appetite is diminished or depraved; the urine scanty, turbid, or milky, (the latter appearance, by the bye, is not noticed by our author;) the bowels are extremely irregular, being either torpid, or affected with diarrhœa, and the evacuations have an unnatural colour and odour. There is generally a distention of the abdomen, and some pain at the epigastrium. The sleep is disturbed and unrefreshing; night-sweats generally attend; and the extremities are apt to become cold. The face is pallid, the features heavy and inexpressive, and the whole skin has a sallow, pasty appearance.

After these symptoms have continued a longer or shorter time, first febricula, and then fever, distinctly come on, and now the acute form may be said to commence. At first there are evening exacerbations, with complete remissions during the fore part of the day; but, in the course of the disease, the remissions become more irregular and imperfect, the paroxysms are more severe, and are attended with delirium and other marks of determination to the head, threatening cerebral effusion. The morbid and irritable condition of the lining membrane of the alimentary canal soon extends itself to the tongue, (which becomes deeply furred,) and is propagated, by continuity, even to the mucous membrane of the nose and trachea. Hence arise that troublesome cough, and that picking of the nose, (in children particularly,) which are such constant symptoms of this complaint. The cough Dr A. calls a "bilious cough," and considers it to be the product of "bilious irritation," acting sympathetically on the lining membrane of the larynx and trachea. We hope that he himself attaches some precise meaning to this "bilious irritation," because, for our part, we cannot, and we are little disposed, at this time of day, to put up with an indefinite term in lieu of an idea. Let us, however, waive his theory, and attend to his facts. He cites several instances where this cough has produced ulceration of the larynx, and subsequent death; and others where it simulated all the symptoms of phthisis, which, however, readily yielded to remedies directed to the function of the liver. Some interesting cases of the latter description we ourselves have met with more than once.

The *causes* of this disease are cold, improper food, such as unripe fruit, pastry, or confectionary, (in which children are so often perniciously indulged,) spirituous liquors, eruptive fevers, and sedentary employments. To these we are inclined to add difficult dentition: but Dr Ayre, with what degree of reason we cannot see, conceives this to be always an effect, and never a cause of marasmus, (p. 21.) That difficult dentition, however, is very frequently an idiopathic affection, and, *per se*, excites febrile disturbance in the whole system, and a local disorder in the alimentary canal, is a matter not of speculative opinion, but of undeniable fact. Not many months ago, we had occasion to see a child, which, for a good many days, had laboured under a harassing diarrhœa, that reduced it to a state of great extenuation. Alternate laxative and astringent remedies of various kinds had been employed to check the looseness, but in vain. On examining the mouth, which had not been suspected as the cause of the mischief, we found the gum at two points in a distended and apparently inflamed state. We forthwith divided it freely with the gum-lancet, cutting down till we felt the instrument rasp on the subjacent teeth. The tension and local irritation being thus removed, the diarrhœa ceased spontaneously *on the following day*, and the little sufferer rapidly became plump and strong. Cases of a similar nature must be familiar to every practitioner conversant with infantile complaints; and it is needless to add, that they overturn the opinion of Dr Ayre,—an opinion, we think, not a little hardy and gratuitous. We can be at no loss to believe, *a priori*, that the irritation of teething should excite commotion both in the organs of circulation and digestion, when we recollect that the fifth pair of nerves and the par vagum arise from the same spot in the brain, and have thus a pretty direct communication with each other. It is ascertained, from many observations, that sympathy is always most close between parts supplied by branches of the same nerves, or by nerves whose trunks either communicate with each other, or arise from the same part of the centre of sensation.

In the diagnosis betwixt marasmus and mesenteric fever, the author lays much stress upon the greater hardness, tumefaction, and tenderness of the belly in the latter than in the former disease, as also on the state of the stools, which, in mesenteric fever, he says, bear a greater resemblance to the ingesta. But we may remark, that these pathognomonic signs are deceptive, and that we have met cases where a diagnosis was absolutely impossible. The difficulty will often arise from both diseases being complicated in the same patient, a thing that not unfrequently happens. Notwithstanding these remarks, we recommend this part of our author's work to be carefully studied.

Dr Ayre has also taken up the diagnosis betwixt acute marasmus and hydrocephalus, (or *phrenitis hydrocephalica*, as it has been called by Sauvage, and which we think a much more correct denomination, in a nosological point of view;) but it is no disparagement to him, when we say that he has left the subject in the same unsatisfactory state in which he found it. In fact, modern writers, of the greatest talent for observation, have confessed the difficulty of an accurate diagnosis; for there are no symptoms, however characteristic of the one disease, that are not, now and then, found to belong to the other. Our author considers sudden screamings, delirium in the day-time, double vision, and the slow, intermitting pulse, as with certainty denoting hydrocephalus; but even to this there are exceptions, though doubtless rare ones. The late Dr Heberden, no mean authority, after enumerating, with his accustomed fidelity, the more striking symptoms of hydrocephalus, adds these memorable words: "Neque tamen perpetuum est ut hæc signa ex hydrope cerebri nascantur; etenim vidi ea omnia in puero cujus cerebrum non nisi consuetâ humoris copiâ humectatum erat, teste peritissimo anatomico qui cadaver inciderat." (*Comment. de Morbor. Hist. et Cur*. p. 186.) In the early stage of either disease, when, from the anxiety of relatives and of the practitioner, a diagnosis is most eagerly desired, we think the following circumstances may assist our judgment. If there are occasional vomitings, with febricula, the tongue, at the same time, remaining clean, there is much reason to apprehend that the primary irritation has its origin and seat in the brain, and that the disease will turn out hydrocephalus. But if, along with the fever and vomitings, the tongue be covered with a thick, creamy, or yellow fur, we are warranted in hoping that the complaint is confined to the digestive organs, and that it will turn out to be only infantile remittent fever. We submit this remark to the attention of Dr A., in the course of his future experience.

In the more advanced stage, fortunately the diagnosis, in a practical point of view, is of no material consequence; for we contend, that the practitioner would be wanting in his duty who should forget that pure marasmus readily may, and often does, induce fatal effusion on the brain,—or who should fail to combat the symptoms by the most active treatment, when they seem to menace that important organ. For our part, when we find a state of general fever existing—the heat of the head greater than that of the rest of the body—delirium by night or by day—vomiting—dilated pupils—intolerance of light—drowsiness—thirst, and the like, we are disposed to care little whether these alarming symptoms arise primarily within the brain, or are secondary to, and symptomatic of, irritation in the primæ viæ, the issue in both cases being alike fatal, if energetic measures be not adopted. We leave the *name* of the disease to be determined by the event; and, in the mean time, employ blood-letting, purgatives, tepid bathing, and blisters. We say we allow the *name* of the disease to be determined by the event; by this we mean, that if it be acute marasmus, our active exertions will generally succeed, but if hydrocephalus, they will as generally fail. The correctness of this last remark is strongly exemplified in the remarkable case related by the late Mr Benjamin Bell, in the 4th appendix to Dr Hamilton's valuable work on Purgative Medicines, p. 225.

This leads us to notice the most serious fault of Dr Ayre's work, namely, his unaccountable neglect in not recommending the vigorous treatment here described, in every doubtful case. His opinions about "bilious irritation acting sympathetically," seem to have taken such a fast hold of him, that to unload the intestines, and, above all, to emulge the liver, are represented as the one thing needful in the cure. This, we assert, is doing the business only by halves; for, though unquestionably these particulars are of great importance, we cannot see why they ought to supersede the use of the lancet in such cases of marasmus as are attended with much pyrexia, or determination to the head. Even a high degree of general fever, independently of any local determination to the head, is in children extremely dangerous, when it continues long; for, owing to the greater size and softness of the infant brain, its parenchyma is apt to yield, and to be irreparably injured, under the impulse of excited circulation. The author pleads his experience against the utility of general blood-letting in acute marasmus; but here we can oppose experience to experience, and state our own conviction that, in children especially, nothing so readily subdues the dangerous symptoms as blood-letting. We may remark, by the way, that in cases of urgency we have not trusted to the slow and too often inadequate evacuation by leeches, but have frequently opened the external jugular vein, a procedure which we wish to see more frequently resorted to by practitioners, inasmuch as it is very easily done, and, moreover, better deserves the old eulogium of "tutò, citò, et jucundè," than any other mode of blood-letting applicable to children.

We next proceed to the chapter on the pathology of the disease, by far the ablest and most original portion of the volume. It reflects great credit on Dr Ayre; and even had he written nothing else, this would have entitled him to the respect of the profession.

He is of opinion, contrary to the respectable authority of Dr Hamilton, (vid. Hamilton on Purg. Med. passim,) that the mere accumulation of feces in the bowels is not, in itself, a cause of marasmus; and that it is not simply by their unloading the intestines, that purgatives are so eminently and rapidly useful in this complaint. To the justice of this opinion we are disposed to subscribe, though for reasons in many respects different from those of Dr A. But before stating our grounds of difference, it is no more than fair to allow him to unfold his pathological views in his own words.

"The view which has been attempted to be taken of the pathology of marasmus, I may here briefly repeat to be,—1. That this disorder consists in a deranged and imperfect action in the secretory function of the liver, and a consequent deficient and unhealthy secretion of bile, as is manifested by the alvine discharges not having that colour which is always imparted to them by it, when it is secreted in a healthy state, and in the proper quantity.

"2. That this derangement in the function of the liver commonly arises from a disorder commencing in the stomach: for the function of digestion is performed by organs whose actions are accordant and co-operative; the healthful action of the liver depending upon a stimulus imparted to it by the stomach.

"3. That in certain deranged states of the stomach, there is either a morbid or an imperfect stimulus given to the liver, by which its secretory function is impeded, and a bilious fluid produced, that is deficient in its quantity, and commonly of a morbid kind.

"4. That, as an interruption in the accustomed actions of a secreting organ occasions a congestion of its vessels, the diminished secretion of the bile gives rise to a congestive state of the vena portarum and its branches.

"5. That in consequence of those efforts which nature makes to free herself from disorder, this congestive state is sometimes spontaneously removed by a copious secretion of bile, constituting the bilious diarrhœa, or the cholera morbus; and that in other cases it is temporarily relieved by an hemorrhoidal flux, or by the discharge of blood from the loaded extremities of the vena portarum; occasioning in this latter case, and when in small quantities, the black and often putrid stools; and when in excess, the idiopathic hæmatemesis, or melæna," &c. &c.—p. 120, 122.

It thus appears, that Dr Ayre holds irregular or imperfect secretion of bile to be the "causa sine quâ non" of marasmus. This opinion is, he thinks, justified by the morbid colour of the alvine evacuations throughout the disease. He argues that, since the feces in a state of health derive their peculiar colour from the bile, any change in that colour must depend entirely upon changes in the biliary fluid. This accounts for the dejections being at first clay-coloured, afterwards dark brown, yeasty, green, or tar like. When the evacuations are perfectly black, he conceives that colour owing not only to vitiated bile, but to an admixture of blood derived from the congested extremities of the vena portæ, and poured by the pori biliarii into the intestines. It is obvious this opinion takes for granted that all the secretions poured into, or supplied by, the bowels, are colourless,—an opinion advanced by our author as a distinct proposition, but which we think quite untenable. We admit that, in a state of health, these secretions are nearly destitute of colour; but under disease the case is far different: *Then*, indeed, the secreting surface of the villous coat of the intestines is deranged along its whole track; and every little gland and mucous follicle pours forth its appropriate secretion wonderfully altered in colour, as well as in other sensible qualities. How else shall we account for the inky stools in the last stage of typhus, or for the quantity of mucous slimy colluvies evacuated almost hourly in dysentery? In marasmus, even upon Dr A.'s own hypothesis, it would require the secretion of bile, instead of being deficient, to be in prodigious excess, to tinge so deeply the almost incredible quantities of feculent matter voided in this disease. That the intermixture of unhealthy bile deepens the colour of the feces, we do not deny; but we maintain that the other vitiated secretions of the alimentary canal have a share equally large in effecting that change.

Besides, it appears to us highly probable, that in this, as well as some other diseases, causes wholly unconnected with the depraved secretions either of the liver or intestines are accessary to the production of morbid stools. It should be recollected that, in every case where the stomach is considerably disordered, digestion is either not at all, or but very imperfectly performed. By consequence, the ingesta pass downwards little changed, and run into fermentation, sometimes of the acescent, and at other times of the putrefactive sort, just as if they were exposed to parallel circumstances of heat and moisture out of the body. When their elements thus re-act upon one another, gases are extricated, some of which (the sulphuretted hydrogen for instance) have the property of communicating a dark tinge to the whole alimentary or fecal mass. Moreover, a large portion of the morbid matter, of all colours, existing in the intestinal contents, during disease, may be reasonably considered as nothing else but varied deposits from one or both of the fermentations above mentioned, in their different stages.

We have descanted the more minutely about the colour of the stools, because this appears to be the greatest, if not the only proof advanced by the author in support of his view of the pathology of marasmus. As to that pathology itself, Dr A. must excuse us when we say, that it is too narrow, and evinces an over-weening partiality for the tenets of the Jatro-hepatici—the pathologists who refer every sort of disorder of the digestive organs to a depraved liver. That the regular action of this mighty gland is of great importance to health, no one will deny; also, that its function is impaired or deranged in marasmus, we readily admit; but we contend that *its* secretion is depraved only in common with the secretions of all the other chylopoietic and assistant chylopoietic viscera; and that the relative importance of its derangement as a cause of the disease (for doubtless it is in part a cause) is by no means paramount, but perhaps of only secondary consequence, when compared with the derangement of the other organs and secretions more immediately necessary to digestion and assimilation.

Upon the whole, we take it, that a derangement of the function of secretion in *all* the chylopoietic viscera, leading in no long time to a weak and irritable condition of these viscera, is the true state of the system in marasmus. As the function of secretion is the direct result of the nervous influence, and as the organs in question are all associated in one nervous union, it is to be expected that, when the function in question suffers, it will suffer contemporaneously in all. From this it follows as a corollary, that our curative efforts should be directed, not to restore the healthy secretion of the liver merely, but to rectify also the vitiated secretions of the stomach and intestines, and to free them from the morbid stimulus of crude unassimilated ingesta, and of acid or putrid feces. Purgatives, by acting upon the entire line of the alimentary canal, and thus giving a salutary stimulus to the whole plexus of abdominal nerves, are admirably adapted to this great purpose.

We were exceedingly pleased with Dr A.'s views of the cause of hematemesis and melæna, and with his judicious directions to treat these complaints by purgative medicines. He conceives the blood in these cases, evacuated by vomiting and by stool, to be derived wholly from the liver. This is very well, but alas! there is nothing new under the sun; the same opinion is advanced in an inaugural dissertation on tropical dysentery published here two years ago;* and, on farther research, we find the same thing, or something nearly similar, stated at pp. 181, 182, of Dr Saunders's excellent work on the liver. In the matter of the treatment by purgatives, too, our author is anticipated by Dr Gasking of Plymouth, and Dr Hamilton of the Royal Infirmary of this place. These little particulars we bring forward from no wish to offend Dr Ayre, or detract from his just merits, but from our inviolable regard to the just principle of "suum cuique."

The chapter on the treatment need not detain us long. Dr A. has been in the habit of trusting entirely to purgatives in this disease, giving a preference, for the most part, to calomel.

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* Dissert. Inaug. de Dysenteriâ Regionum Calidarum. Auct. Archibald Robertson. Edinburg. 1817. The words are, "Admodum credo particulas sanguinis rubras è minutissimis Venae Portae ramulis absque mutatione detrudi, et demùm in poros biliarios effundi. Ibi sanguis cum bile inguinatâ miscetur, et citò in canalem alimentariam transit. Color sanguinis nigerrimus huic opinioni de ejus origine haud parum favet; quia bene novimus sanguinem Venae Portae aliarum venarum sanguine nigriorem." p. 29.

In adults there is less need for discrimination in the choice of the purgative, but when children are the subjects of this complaint, drastic ones should be carefully avoided, on account of the greater irritability of the infant habit. Even calomel, in such cases, will be found too irritating, unless its doses are followed up in an hour or two by castor oil. In our own practice, we are in the habit of triturating the calomel with chalk, and exhibiting it in the proportion of one or two grains of the former, to four or six of the latter. This combination seems to render the calomel less nauseating, and obviates its tendency to gripe: whether this mitigation be owing to the chalk's absorbing and neutralizing the free acid in the primæ viæ, or to some more obscure influence, we are not prepared to say. The tepid bath, also, in such cases, has admirable effects in allaying irritation.

Cholera in persons of all ages, the author treats with half a grain, or one-third of a grain of calomel, repeated at short intervals. We doubt much whether these minute doses possess any superiority over the larger and more active ones. We ourselves have very successfully employed the latter, generally, however, in combination with opium.

We give the greatest praise to Dr A. for the earnestness with which he has enforced a daily minute inspection of the alvine discharges. It is only by watching the changes they spontaneously undergo, and the advances they make to their natural colour and fetor, that the physician can judge of the effect of his remedies, or of the progress of convalescence.

Though we have been constrained to dispute some of our author's leading tenets,—of his book, as a whole, we would be understood to speak very favourably. It manifests both talent and observation. Its subject is comparatively new; for, until the useful works of Dr Hamilton * and Mr Abernethy, † the influence of depraved digestive organs on general health and local disease was very imperfectly comprehended, even by the best informed of the faculty. But the inquiry has now obtained that full attention its importance deserves; and Dr Ayre is entitled to the praise of having added to the facts and practical observations collected by the two eminent individuals just named.

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* "Observations on the Utility and Administration of Purgative Medicines, in several diseases." Edinburgh, 1805.

† "Surgical Observations on the Constitutional origin and treatment of Local Diseases." London, 1806.

III.

Pathological and Surgical Observations on Diseases of the Joints. By B. C. Brodie, F. R. S. Assistant Surgeon to St George's Hospital, and Lecturer on the Theory and Practice of Surgery. 8vo. pp. 329. London, 1818.

In this volume, Diseases of the Joints are classed pathologically, according to the particular tissue which is first implicated. Thus they come to be divided into those that arise, 1st, from various morbid states of the synovial membrane; 2d, from ulceration of the cartilages covering the articulating surfaces of bones; 3d, from a scrofulous affection, originating in the cancellous structure of the bones themselves; and, 4th, from inflammation of the bursæ mucosæ lying over, or immediately contiguous to, a given joint. There is a chapter towards the end of the book on curvature of the spine, (from disease of the bodies of the vertebræ themselves, or of their interposed cartilages,) a topic, it must be admitted, sufficiently allied to the principal subject of the work, to allow of being considered in immediate connection with it.

The morbid states of the synovial membrane are inflammation, ulceration, and organic change of structure. Of these the first is by far the most frequent, and the author traces its symptoms with sufficient precision; but as they are principally such as are to be expected from pure inflammation, we may be spared the necessity of dilating upon them in this place. Yet we ought to notice the increased secrection of synovia which always takes place when the membrane is acutely inflamed. This effused fluid increases the general swelling, and when it has accumulated in the joint, its fluctuation can be distinctly felt externally.

The pain, tension, and symptomatic fever in the early stage of inflammation of the synovial membrane of a joint, are always much more considerable than where ulceration of the cartilages is the primary disease. Besides, as the author has well observed, though the pain on any general motion of the joint is greater, the pain on rubbing the articulating surfaces strongly against each other is much less than when the cartilages are in process of ulceration. Hence, in the former (viz. inflammation of the synovial membrane) the limb can support the weight of the body without much uneasiness, whereas in the latter (viz. ulceration of the cartilages) any attempt to throw the weight of the trunk on the diseased limb causes very great pain. From repeatedly observing these facts, Mr B. advises, in every case of suspected hip disease, that we should press the articulating surfaces of the affected joint strongly together, as this will materially assist our diagnosis. In the true morbus coxarius,

> " if the patient be placed on an even surface, in a horizontal position, and the hand of the surgeon be applied to the heel, so as to press the head of the femur against the concavity of the acetabulum, violent pain is the consequence; although this be done in so careful a manner, that not the smallest degree of motion is given to the hip-joint. This circumstance is well deserving of attention; and no one should attempt to give an opinion as to the nature of a disease connected with the hip, without having made an examination in the manner which has been just described." P. 141.

The treatment recommended by the author for inflammation of the synovial membranes is certainly good; yet we wish he had laid more stress upon general blood-letting in addition to cupping and leeching. Cases of this affection not unfrequently occur where the local inflammation is so severe, and the general fever so violent, that nothing short of very decisive general as well as topical depletion can quell the symptoms. We are convinced that when cases of such severity are given over to the ordinary routine of leeches, fomentations, and so forth, death, or at least incurable lameness, will often be the result. That treatment is frequently the most inert, where, as the phrase is, "*every thing is done*;" because, according to this mode, every remedy, although but of secondary or third rate importance, obtains in its turn a fair trial and its share of trust, while much valuable time is irrecoverably lost, and the really active measures, which alone held out a fair prospect of efficacy, are either superseded, or used with an insufficient degree of vigour and perseverance.

Mr Brodie relates some uncommon cases where an attack of gonorrhœa was attended with a purulent ophthalmia, and a painful swelled state of the joints of the upper, and especially the lower extremities. This he considers to be an affection of the synovial membranes, probably from extension of the inflammatory irritation. We ourselves have met with more than one instance of the like kind; but we always looked upon the articular affection (we know not whether rightly or not) as simply wandering rheumatism, accidentally co-existing with the gonorrhœa. What led us to adopt this opinion was, that the subjects of the disease were men of intemperate habits, and had been, as they themselves familiarly expressed it, *keeping it up* for several days before the symptoms appeared. It is not to be wondered at, that exposure to night-dews, after being debilitated by venery and excessive drinking, should have induced an attack of rheumatism. In the cases we have seen, the digestive organs were always greatly deranged, the pulse from 96 to 108, the tongue deeply furred, and the belly costive. The complaint proved tedious, but ultimately yielded to purgatives, small doses of pulv. ipecac. comp. and remedies directed to restore the healthy tone of the stomach and bowels. We beg to put it to Mr Brodie's recollection, whether the cases he treated also occurred in persons addicted to intoxication. It appears that he found the *vinum colchici* more successful than any other remedy in removing this painful and tumefied state of the joints.

In order to promote absorption of the fluid effused in cases of inflammation of the synovial membranes, the author advises repeated blistering and frictions. For the latter purpose he thinks the liniments of our pharmacopœias too weak,—(a circumstance which we have long been convinced of,) and recommends the following in their stead.

"℞. Olei olivae ℥iss:
Acidi sulphurici ʒss. M. Fiat linimentum." P. 35.

He adds a judicious caution, that friction should not be employed too freely, as it sometimes causes a return of inflammation in parts where the inflammatory disposition may still linger. When this untoward effect results, the friction should be laid aside, and topical bleedings renewed.

He comes next to state his opinion on the pathology of morbus coxarius; the primary morbid cause, he conceives, is ulceration of the cartilage covering the head of the femur and the concavity of the cotyloid cavity.

> "Ulceration of cartilage may be the consequence of inflammation of the cartilage itself, or of the bony surface to which it is connected; but in many instances there are no evident marks of inflammatory action having preceded it, either in one part or the other; and the inflammation which afterwards takes place, appears to be rather the attendant on, than the cause of, the ulcerative process. The ulceration of soft parts is usually, and, as far as I know, always, attended with the secretion of pus; but it is otherwise with the articular cartilages, in which suppuration seldom takes place, while the ulcer is small, and often the disease proceeds so far as to cause caries of the bones to a considerable extent, without matter being formed in the joint. This circumstance is deserving of notice. It has been long established, that suppuration may take place without ulceration; and it appears, that, in this instance, ulceration may take place without the formation of pus." P. 101, 102.

That ulceration may sometimes take place without the formation of pus, we are willing to concede to our author; but that it ever takes place without being preceded by inflammation, is what we cannot possibly conceive. On the contrary, we are accustomed to think, that, in every case of this sort, a languid inflammation, such as is incidental to scrofulous habits, precedes the process of ulcerative absorption, and by its presence urges on the progress of destruction in the cartilages and bones of the joint. What is termed *the scrofulous diathesis* manifests itself, for the most part, by this tendency to slow and insiduous inflammation. It generally makes its appearance in that part of the body which is either originally weakest, or which has been rendered so by the application of cold, or other causes of an injurious tendency. In this way the local disease is often merely an index, or topical outlet of the constitutional disorder, which, if restrained in one quarter, is extremely apt to break out in another, under a different form. As illustrating these opinions, we may state the case of a young lady that came under our observation within these twelve months. She had for a length of time laboured under well-marked symptoms of the morbus coxarius. These had even gone so far as to impair her general health, and induce hectic fever. At the distance of seven months from the first appearance of the complaint, the disease in the hip underwent a spontaneous cure,—we say *spontaneous*, (for it would be too much to attribute this favourable change to the caustic issues, &c. which had, for the six preceding months, been tried in vain ;)—her relations flattered themselves that she would do well. But ere long symptoms of confirmed phthisis occurred, and in a few weeks put a period to her life. Now, we regard the constitutional cause of both diseases, in this case, to have been one and the same ; and are inclined to believe, that there was a conversion of the scrofulous inflammation of the hip into a similar affection of the lungs, which were tuberculous ; and that the occurrence of the latter was the causeof the cessation of the former, which, but for this occurrence, would in itself have proved fatal.

We can assure Mr Brodie that, though we have thus taken the liberty to oppose an opinion to an opinion, we mean not to dogmatise on the subject. Much is yet to be learnt, and, upon the whole, the matter is one that must be committed to future investigation.

The following account of the progression of the symptoms is well drawn.

"1. Ulceration takes place in the cartilages generally in that of the acetabulum first, and in that of the head of the femur afterwards: sometimes it begins in both at the same time.

"2. The ulceration extends to the bones which become carious; the head of the femur is diminished in size, and the acetabulum is rendered deeper and wider.

"3. Abscess forms in the joint, which after some time makes its way by ulceration through the synovial membrane and capsular ligament into the thigh, or nates, or even through the bottom of the acetabulum into the pelvis. Mr Astley Cooper has shewn me two specimens, in each of which the abscess had burst into the rectum.

"4. In consequence of the abscess, the synovial membrane and capsular ligament become inflamed and thickened. The muscles are altered in structure; sinuses are formed in various parts, and at last all the soft parts are blended together into one confused mass, resembling the parietes of an ordinary abscess." P. 120.

In the advanced stage of this dreadful disease, either the acetabulum becomes absorbed, and allows the head of the femur to slide upon the dorsum of the ilium, or the head of the bone itself is destroyed, and thus admits of the femur being drawn upwards by the action of the muscles. In this way the shortening of the limb is sufficiently accounted for. But, previous to this, there has been observed an opposite symptom, namely, elongation of the limb, which has greatly puzzled pathologists. Our author thinks that this elongation is apparent only, and caused by obliquity of the pelvis:

"The apparent elongation is produced by the position of the pelvis being altered in such a way, that the crista of one ilium is visibly depressed below the level of that of the other. It is easy to understand how this effect is produced, by observing the position in which the patient places himself when he stands erect. He supports the weight of his body on the sound limb, the hip and knee of which are in consequence maintained in the state of extension. At the same time the opposite limb is inclined forward, and the foot on the side of the disease is placed on the ground, considerably anterior to the other, not for the purpose of supporting the superincumbent weight, but for that of keeping the person steady, and preserving the equilibrium. Of course this cannot be done without the pelvis on the same side being depressed. The inclination of the pelvis is necessarily attended with a lateral curvature of the spine, and hence it happens that one shoulder is higher than the other, and that the whole figure is in some degree distorted." P. 146, 147.

We are satisfied this view of the subject is perfectly correct. Indeed, this very opinion has been advanced by Mr John Hunter, and farther illustrated by Mr Crowther in his work on White swelling, published in 1808, though Mr Brodie neither refers to the one or other of these gentlemen

We regard as one of the novelties of the age, that over-confident spirit by which too many modern writers erect themselves into independent observers, and treat their subject with all the *hardiesse* of seeming originality, just as if it had never been written upon—and well written upon, before. Instead of being at pains to consult the labours of their predecessors, 'tis odds but they endeavour to make a merit, in the eyes of the reader, even of their negligence; and then we are sure to be treated with a few stately periods touching the tyranny of great names, and the mischief of slavish obedience to the tenets of books!

Yet those who act upon this cavalier principle are surely guilty of inconsistency, if not of presumption: for who, think they, are to read *their* books, when they will not do the like favour to others? If *all* were to imitate their contempt for the writings of those that have gone before us, what, in a short time, would be the fate of their own lucubrations?

These are general remarks, and we state them without intending any particular reference to Mr Brodie. On the contrary, we believe him to be a writer of a far better stamp; yet, honestly speaking, there are, for so well-informed a writer, marvellously few acknowledgments to those authors who have already treated so ably of diseases of the joints.

The following remark on caustic issues will be read with great pleasure; it bespeaks the man of experience.

> "I have seen several cases in which the caustic issue has, in the first instance, removed all the symptoms of the disease, and yet, after some time, notwithstanding the patient has remained in a state of perfect quietude, and there has been no evident cause of aggravation, they have returned nearly in the same form as before, and with their original severity. In some of these cases their recurrence is to be attributed to the issue itself, which, from some cause that the present state of our knowledge does not enable us to explain, produces an effect apparently the opposite to that which it produced when it was first made. The issue being allowed to heal, the symptoms again subside, and, perhaps, the patient may find himself entirely and permanently relieved before the sore is completely cicatrized. The same thing may be observed, perhaps more frequently, where a blister has been long kept open by means of the savine cerate; and here, if the blister be of a large size, the recurrence of the pain is usually attended with a furred tongue and much constitutional irritation, of all which the patient is relieved when the blistered surface is allowed to skin over." p. 179.

We are somewhat surprised that the author has not employed corrosive sublimate as an escharotic, after the manner recommended in a communication at page 432 of the fifth volume of this Journal. We know that, in the hands of various practitioners, it has been successful. We think also that the application of the Moxa, in the manner recommended by Baron Larrey, and other Continental surgeons, deserves a more extensive trial, in chronic diseases of the joints, than it has hitherto met with in this country.

We have now given an account of the principal contents of this work. The remaining two chapters are on Curved Spine, and Inflammation of the Bursæ Mucosæ; but they do not suggest any particular remark.

We conclude by recommending the volume before us as a useful one; but we must confess, that, as a whole, we found it neither so interesting nor instructive as Mr Brodie's known ability and opportunities led us to expect.

IV.

Physiological and Medical Researches into the Causes, Symptoms, and Treatment of Gravel. By F. MAGENDIE, Doctor in Medicine of the Faculty of Paris, &c. &c. Translated by a Member of the Royal College of Surgeons in London. Longman and Company, 1818. 12mo, pp. 103.

THE principal novelty of this book consists in the author's recommending abstinence from animal food in the treatment of calculous affections, on the new ground that such diet is injurious, by furnishing the azote which is essential to the production of lithic acid. His treatment is, in fact, the converse of that suggested by Mr Rollo in diabetes, and it produces frequently a discharge of urine, the copiousness and qualities of which indicate a condition of the system nearly approaching to that of this latter malady.

"I have myself," he tells us, "analysed the calculous concretions voided by patients affected with gravel, and in every instance, above thirty in number, found them composed of uric acid, united with a small proportion of animal matter. Thus overlooking, or considering as exceptions, the very rare cases where gravel has been found wholly, or in part, composed of oxalate and phosphate of lime, cystic oxide, &c. we may state, that this substance is constantly composed of uric acid, united with a small proportion of animal matter, which is probably the mucus that lines the urinary passages." pp. 7, 8.

It is, accordingly, to the chemical composition and production of lithic * acid, that the author, in the first instance, di-

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[*] We consider this name as far preferable to that more generally employed, of *uric* acid, for the reasons mentioned by Dr Marcet, (on Calculi, p. 59,)—the latter name appearing objectionable, "not only because the substance in question is found in gouty concretions as well as in urine, but also on account of the close resemblance which the term *uric* bears to that of *urea*, another and most characteristic constituent of urine totally distinct from this."

rects his attention, and he takes the proportions of the elements of that substance from the experiments of M. Berard, which agree with those of Dr Prout, published in 1817,* in assigning to its composition about 40 per cent., by weight, of azote. We prefer, however, the result of the more recent experiments of the latter gentleman,† which give, for the elements of lithic acid, the following proportions in 100 parts by weight, viz.:—hydrogen, 2.22; carbon, 40.00; oxygen, 26.66; azote, 31.11. It is obvious, that, to cut off the supply of any one of these elements, would effectually prevent the formation of the lithic concretions, which are composed of them. But M. Magendie is of opinion, that the production of lithic acid depends principally upon the quantity of *azote* introduced into the system, and, consequently, upon the quantity employed of aliment which contains that principle, having discovered, he tells us,

> " from a series of experiments, of which I last year presented an account to the Academy of Sciences, ‡ that, if a carnivorous animal be deprived of all food containing azote, and fed with sugar, gum, or oil, substances known to be nutritive, but which do not contain azote in their elements, his urine, at the end of three or four weeks, will be entirely devoid of uric acid." p. 10.

The experiments here alluded to were tried upon dogs, several of which were sacrificed in the course of the author's inquiries; and, notwithstanding the ingenuity of his deductions, we really cannot regard the information thus obtained as sufficient to counterbalance the sufferings of these unfortunate creatures. Indeed, we believe, that, in almost every instance where living creatures have been tortured scientifically, the results derived might have been gained by measures less revolting to humanity. We extract, however, the following passages, as no account of these experiments of M. Magendie has yet appeared in our pages.

> " A small dog, three years old, fat, and in good health, was confined to white sugar alone for food, and distilled water as drink, of both which he was allowed to take as much as he chose. For seven or eight days he appeared to do very well with this new diet; he was active and docile, and ate and drank with his usual avidity. In the second week he began to get thin, although his appetite continued

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* Med.-Chirurg. Trans. Vol. VIII. p. 539.

† Ibid. Vol. IX. p. 477, 478.

‡ Memoir on the Nutritive Properties of Substances which do not contain Azote, read to the Acad. of Sciences, &c. Aug. 1815. An abstract of these experiments is given also in the author's " Precis Elementaire de Physiologie," Vol. II. p. 390, &c.

good; and he ate from six to eight ounces of sugar in 24 hours. His alvine evacuations were neither frequent nor copious, but his urine was very abundant in quantity. He became still more thin during the third week; his strength diminished, he lost his activity, and his appetite was much diminished. At this time there appeared, first on one eye, and afterwards on the other, a small speck of ulceration on the centre of the transparent cornea, which increased rapidly, and at the end of a few days had become more than a line in diameter. The depth of it increased in the same proportion. Soon afterwards the cornea became entirely perforated, and the humours of the eye were evacuated externally. This singular phenomenon was accompanied with a copious secretion from the glands of the eye-lids. The emaciation continued to increase, and the strength of the animal to diminish, and, though he ate three or four ounces of sugar daily, he became so weak, that he could neither chew nor swallow. *All other motions were, for stronger reasons, impossible. He died on the thirty-second day of the experiment.*" pp. 10, 11, 12.

The urine of this miserable creature, as well as of some other victims of experiment, instead of being acid, as it is in all carnivorous animals, was sensibly alkaline, and did not afford any traces of lithic acid, or of the phosphates.

The pathology of gravel is given very distinctly by M Magendie, in five very short chapters,—the whole of the phenomenon arising, according to him, from the want of balance between the solvent power of the urine and the quantity of lithic acid which it contains, in consequence either; 1*st*, Of an absolute increase in the quantity of acid; or, 2*d*, Of a diminution of the quantity of urine; or, 3*d*, A reduction of the temperature, and thence of the solvent power of that fluid. The author adds, however, that he is far from considering these three causes, separately or united, as the only one which may produce gravel; but they must, he conceives, be considered as the principal ones, according to the present state of our knowledge.

The remote causes may be reduced to the following:—

" 1. Mature and advanced age.

" 2. Too nutritious diet, principally composed of substances containing a large proportion of azote.

" 3. The want of sufficient exercise, literary labours, lying too much in bed, &c.

" 4. A habit of drinking but little of liquids of any kind.

" 5. The use of generous wines and spirituous liquors.

" 6. Copious perspiration, and all serous evacuations, occurring in persons otherwise disposed to gravel.

" 7. The ill habit of long retention of the urine in the bladder." pp. 41, 42.

And the indications of cure are, 1*st*, To diminish the quantity of lithic acid formed in the kidneys. 2*d*, To increase the secretion of urine. 3*d*, To prevent the solidification of the lithic acid, by saturating it. And, 4*th*, In the case of calculi being formed, to favour the expulsion of them from the bladder, and to attempt their solution. It is under the first of these heads, as we have already stated, that the novelty of M. Magendie's treatment will be found; and he asserts, that nothing more is required, in general, to effect the object of this indication, than a restriction of diet with respect to the use of substances which contain azote. In certain cases it will, he thinks, be necessary to dispense entirely with food of this description; but he has often seen persons cured of the deposition of red sand merely by ceasing to take animal food at breakfast, to which they had been accustomed

The following case is given by the author as demonstrating remarkably the connection of gravel, or (which he considers as the same thing) of an excess of lithic acid, with the mode of life and the nature of the diet; but it would evidently be going too far to assert, that the effects here stated arose entirely from the diminished proportion of animal matter in the food.

"M. ———, a merchant, residing in one of the Hanseatic towns, was possessed, in the year 1814, of considerable property, and lived in the style of a man of consequence. He kept an excellent table, in the pleasures of which he indulged without restraint. He was at this time severely afflicted with gravel. An unexpected political event occasioned the loss of his whole fortune, and obliged him to seek refuge in England, where he passed more than a year of almost absolute poverty, from which he suffered the most extreme privations. His disorder completely disappeared during this period. His affairs afterwards became re-established; he resumed his former mode of life, and he again became tormented by his old disease. A second reverse of fortune again deprived him of his property; he went to France almost without resources. His mode of life was conformable to his pecuniary means. The gravel again disappeared. At length industry again placed him in easy circumstances; he followed his inclination for the pleasures of the table; and it was then he consulted me, in consequence of the return of his complaint." * pp. 22, 23.

Some other instances also are stated of considerable benefit from change of diet alone, among which that of a provincial magistrate, a great lover of the pleasures of the table, is remarkable, (p. 58, &c.;) and, in this case, after 12 or 15 days of the treatment, the patient "made a very large quantity of urine, quite disproportionate to that of the fluids which he drank." But that which bears more directly on the author's theory is the case of a lady, who, having been for several years subject to gravel, was induced to eat large quantities of sugar, and was entirely relieved from her malady in about six weeks, but became again afflicted with it on resuming animal food.

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* "He was at the same time affected with gout, which constantly appeared and disappeared with the gravel."

The theory by which the author accounts for this connection between diet and calculous affections is as follows:

> "The muscular system is that of which the nutrition is the most rapid, and which most abundantly consumes nutritive substances, when it is often put in action. Thus, all persons who are to use great exertions of the muscles previously eat freely of aliments containing azote. If the same kind of food be taken in considerable quantity, without much action of the muscular organs, azote becomes abundant in the system, from those organs not appropriating to themselves the nutritive matter, and is directed to the kidneys, the principal emunctory of azote; it is there transformed into lithic acid, and tends to the production of gravel. As long as the quantity of urine is sufficient to dissolve the uric acid that may be formed, the increase in the proportion of this substance is not productive of inconvenience, and this, without doubt, is what takes place in persons who use the kind of diet just described, and are not affected with gravel: but, if the proportion of the urine does not increase in a ratio with that of the acid, or if it become less, the appearance of gravel must be expected." pp. 24, 25.

We shall not follow M. Magendie through his remaining indications, but we should have been glad to have had room for an extract which he has quoted from a paper of the celebrated Mascagni, in the "Memoirs of the Italian Society" for 1804, where that writer gives an account of the successful use of carbonate of potash in his own case. The art of employing the alkalies and alkaline earths

> "consists," M. Magendie thinks, "in continuing their use as long as they are efficacious, and in ceasing to do this, as soon as any unpleasant effects appear; and, lastly, to vary them with judgment, according to circumstances; but, whichever of them may be employed, its evident effect should be an alkaline state of the urine, without which nothing can be expected from them in the cure of gravel, at least no result which can be theoretically accounted for will be produced." pp. 79, 80.

The author thinks it very possible to effect the solution of *lithic* calculi in the bladder, and even in the kidneys or ureters, and he strongly recommends the repetition of experiments upon this subject; but, in every such case, long perseverance in the method of treatment will be necessary. It is remarkable that he considers dyspepsia not as one of the causes of calculous affections, but as the simultaneous effect of the same agents, (p. 36;) and he conceives that anti-dyspeptic medicines, such as purgatives, cold and sulphureous baths, or fumigations, change of residence, occupations, &c. produce relief in a manner not hitherto explained. Where the calculus or gravel is composed of phosphoric salts, the best treatment is, to cause an abundant secretion of urine, in order to favour the solution of the salt, and to relieve the weakness and distress which always accompany this disease.

The attention which we have given to this little volume has been proportioned rather to the importance of the subject, and the high reputation of the author, than to its bulk; but, though we think very highly of the ingenuity which the work displays, and strongly recommend it to the perusal of our readers, we consider some of the leading doctrines of M. Magendie as very questionable; and, admitting the originality of the principle upon which he is induced to forbid the use of animal food in calculous affections, it is quite certain, that the same method of treatment had been recommended, in substance, several years before the date of his experiments upon this subject.

1. Dr Marcet has stated, that concretions of the lithic kind, far from being the only species of *calculi*, do not even constitute one half of the concretions which are formed in the urinary passages, and his estimate was taken from very extensive collections, the relative numbers of the different species being as follows, viz.

	Norwich Hospital.	Guy's Hospital.
Lithic calculi, -	66	22
Fusible, - -	49	24
Mulberry, - -	41	22
Other species, -	25	19
Total number, -	181	87

In Dr Marcet's own collection also, of 50 specimens, the proportions agreed very nearly with those of the collection at Guy's Hospital.* Unless, therefore, we suppose the composition of *gravel* to differ very materially from that of the larger urinary concretions, the opinion of M. Magendie as to the great frequency of lithic gravel must be incorrect.

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* Marcet on Calculi, pp. 58—109, &c.

2. The connection between the production of lithic acid and the quantity of animal matter taken in by different animals was distinctly pointed out by Dr Wollaston in his Memoir on the Cystic Oxide, published in the Philosophical Transactions for 1810; and we have the greater satisfaction in quoting the following passages from that valuable paper, because, although the facts are alluded to by M. Magendie, the inference of Dr Wollaston, as to the probable effects of food in calculous diseases, is altogether passed over;—a degree of inattention which the flagrant injustice (to give it the most gentle name) of Fourcroy, in attempting to claim for himself and M. Vauquelin the previous discoveries of Dr Wollaston on the composition of calculi, renders the more remarkable.

"There is one suggestion," Dr Wollaston mentions, "*with respect to the prevention of calculous complaints*, so nearly connected with the present subject that I think it may deserve to be recorded. Since the white matter contained in the urine of birds which is voided along with their dung, has been remarked by M. Vauquelin to consist principally of uric acid, I have paid some attention to the different proportions in which this matter is voided by different species of birds, to see how far it accorded with the different qualities of their food. And I found that, in the dung of the goose, feeding wholly on grass, the proportion did not seem so much as $\frac{1}{200}$ of the whole dung. In that of a pheasant, kept in a cage, and fed on barley alone, it was about $\frac{1}{14}$ part. In that of a hen, having the range of a garden and farm yard, and, consequently, procuring insects and possibly other animal food, the proportion was manifestly much greater, and combined with lime. In the dung of a hawk, fed upon flesh alone, the quantity of matter voided in a solid state bears but a small proportion to the residuum of uric acid that is left by the urine when dry. And in the gannet, feeding solely on fish, I have observed the evacuations in some instances to be mere urine, for it contained no solid matter excepting the uric acid.

"*It seems, consequently, deserving of inquiry, what changes might be produced in the urine of any one animal by such alterations of diet as its constitution would permit; for, as far as any inference can be drawn from these varieties which naturally occur, it would appear that persons subject to calculi, consisting of uric acid, as well as gouty persons, in whom there is always a redundance of the same matter, have much reason to prefer vegetable diet;* but that the preference usually given to fish above other kinds of animal food is probably erroneous."

3. The opinions of Dr Wollaston are confirmed by the more recent discoveries that the calculi of graminivorous animals are destitute of lithic acid; * and that the excrements of the Boa

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* Marcet, pp. 139, 140.

constrictor, [*] and of some other species of serpents [†] which feed almost exclusively on animal matter, consist either wholly or in large proportions of that substance. The experiments above-mentioned of M. Magendie tend to a similar conclusion with respect to the human species. But, on the other hand, the validity of this inference is rendered doubtful by the recent statements of Mr Hutchison, [‡] that calculous affections are of exceedingly rare occurrence among seamen, whose diet, nevertheless, consists in a very large proportion of animal food, though, it must be admitted, that salt provisions may differ materially in their effects upon the urinary excretion from fresh meat.

4. But, supposing the fact to be established, that animal food contributes to the formation of lithic gravel and calculi, it is, we think, by no means clear, that this effect depends upon the quantity of azote which is thus conveyed into the system. We are, on the contrary, disposed at present to believe, that the effects of diet, as well as those of certain medicines which are known to be of service in calculous affections, arise from their acting on the *digestive organs*, in some manner which has not hitherto been explained: the efficacy neither of the acids nor alkalies being referable solely to their chemical action; and magnesia producing little good if it does not act as a purgative, [§] while the connection between the organs of digestion, and the composition of the urine in the human species, is evident from the curious fact observed by Mr Rose, [||] and confirmed by Dr Henry, [¶] that in hepatitis neither urea nor lithic acid are found in that secretion. In diabetes also, the urine is almost destitute of both these principles; [**] and the statements just alluded to of Mr Hutchison render it probable, that the exemption of seamen from calculous diseases is connected with the freedom from dyspeptic affections which he states that class of persons to enjoy.

On the whole, therefore, we concur with Dr Marcet, whose authority upon this part of the subject will not be questioned, in regarding the treatment of calculus, (whether in the form of gravel or of stone,) as being very different from a question of pure chemistry; and we agree also with Dr Prout, whose accurate experiments give great weight to his opinions, in taking that to be the best treatment which tends most directly to restore the *general health*, with which object the state of the digestive functions will always speedily demand the greatest attention of the physician.

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[*] Dr Prout, in Thomson's Annals, &c. June, 1815, Vol. V.

[†] Dr J. Davy, Phil. Trans. 1818, p. 303, &c.

[‡] Med. Chirurg. Trans. Vol. IX. p. 443, &c.

[§] Prout, in Med. Chirurg. Trans. Vol. VIII. p. 547.

[||] Thomson's Annals of Philosophy, Vol. V.

[¶] Ibid. Vol. VI. p. 392, 3.

[**] Henry, Med. Chirurg. Trans. Vol. II. p. 123. Berzelius states the quantities in 1000 parts of healthy urine to be of urea about 30, and 1 of lithic acid. Med. Chirurg. Trans. Vol. III. p. 271.

V.

Memoir on the Formation and Connections of the Crural Arch, and other parts concerned in Inguinal and Femoral Hernia. By Robert Liston, Member of the Royal College of Surgeons of London and Edinburgh; Lecturer on Anatomy and Surgery, &c. &c. &c. 4to, pp. 22. Edinburgh, 1819.

The importance of a correct knowledge of the anatomy of the groin in relation to crural and femoral hernia, has induced many of the most celebrated surgeons to examine its structure with great attention, and to describe it most minutely. But its nature is so complex, and the connection of its various parts is so obscure, that Mr Liston has thought it necessary to examine the anatomy of the crural arch anew, and to correct various errors which prevail regarding it.

One object of his publication is to show, that the ligament of Gimbernat, or rather the crural arch, is totally unconnected with that of Poupart, and that the arch is composed of an union of fasciæ which exist in nature, and are not at all produced by the mode of dissection. We have repeatedly seen Mr Liston dissect the parts, and are convinced that his views on both points are correct.

Poupart's ligament, as it is called, is in fact the lower boundary of the external oblique muscle of the abdomen, thickened, and extending from the anterior spinous process of the ilium, to the tuberosity of the pubes. It lies between the fascia comprising the crural arch, and is connected with it only by loose cellular substance, so that Poupart's ligament may be divided across without much relaxing the crural arch, or enlarging the crural ring, of which the arch forms the upper boundary.

The crural arch is composed of an union of fasciæ, 1st, of the subcutaneous aponeurosis which covers the muscles of the abdomen, and of the inner part of the thigh, forming the external layer; 2d, of the transverse fascia which lines the abdominal muscles, forming the internal layer; 3d, Mr Liston adds to these a middle layer, " formed by the attachment of the combined tendon of the internal oblique and transversalis to the crest of the pubes and the descending fibres, strengthened by a strong cross slip from the attachment of the rectus abdominis of the opposite side." By the union of the external and internal layers chiefly is formed a sharp and strong semi-lunated edge, rising from the outside of the vena saphena, at its junction with the femoral vein, and descending to the spine of the pubes. The lateral portion of the crural arch is the falciform process of the fascia lata, and the mesial portion is the crescentic portion of some authors. In the fold between the external and internal layers of the crural arch, the thickened edge of the tendon of the external oblique, or Poupart's ligament, is inclosed, and between the splitting of the layers at the mesial end of the arch, the united tendon of the internal oblique and transversalis dips to be inserted into the pubes.

> " From this view of the parts, it will immediately be understood how the Crural Arch can be relaxed, and the reduction of Femoral Hernia, either before or during the operation, facilitated, by the bending and rotation of the thigh inwards, or the carrying of the one limb across the other. This relaxation of the arch will account for the frequent occurrence of Femoral Hernia, in people falling under great weights; in which case the knees are commonly forced together. The ligament of Poupart cannot possibly be at all affected by the motions of the inferior extremity, as extending betwixt two immoveable points of bone. Whereas the tension of the Crural Arch, formed as has been described, is changed in every position. It will clearly appear, also, that the division of the ligament of Poupart alone, though it may slightly weaken the Crural Arch, cannot be trusted to as a means of relieving strangulation. The division of the falciform process, on the other hand, or its continuation, the femoral ligament, must immediately relax both the arch and its crescentic portion, and by attention to the position of the limb, it will seldom be found necessary to carry the edge of the knife against either of the two last mentioned parts."

For other details we must refer to the work itself, which is illustrated by three very spirited sketches, from the pencil of Staff Surgeon Schetky, which induce us to take this opportunity of recommending to the younger part of our readers to cultivate the art of drawing, which they will find of great advantage to them, not only as rendering them independent of the assistance of an artist when they wish to procure a delineation of natural or morbid structure, but still more so as improving the accuracy of their eye, in regard to form and colour.

After we have bestowed so much praise upon the matter of Mr Liston's treatise, it may seem inconsistent or frivolous to find fault with its manner. But the manner is of importance to the success and utility of such a work, although we may readily excuse ignorance of the art of authorship in a first performance. In the composition of his treatise, Mr Liston has erred in writing as if his readers were well acquainted with the anatomy of the parts. He should have begun by giving an exact and perspicuous account of the true structure, in order to enable his readers to understand his criticisms of the descriptions of his predecessors, and his physiological and pathological remarks, whereas we are left, in a great measure, to acquire a complete knowledge of his views from incidental observations, introduced on different occasions. It is also to be regretted that the explanation of the plates was not so printed that the whole might have been contained in one page, opposite to the plate, as the being obliged to turn for so many references is extremely troublesome. The position of the figure upon the second plate, in which the mesial line runs diagonally, is excessively awkward, and renders the whole difficult of being comprehended. The reversed position of the parts upon the third plate is also a disadvantage; and in general, when different views of the same part are given in several plates, it would tend much to facilitate our conception of their relative connection, if their position upon each were the same, or as nearly so as the subjects will permit; and we even recommend the use of the same letters to designate the same parts, when they occur in the different dissections, as tending greatly to make them more easily understood.

VI.

Lettere del Professore Scarpa *al* Professore Maunoir, *concernenti l'Opera del* Signior Adams *sulla Cateratta, e sulla pupilla artificiale.* 8vo. Milano, 1818.

(Concluded from No. 59, p. 308.)

LETTER IV.

Pavia, 22d January 1818.

I have still a few remarks to make with regard to the cataract *adherent* to the posterior surface of the iris, whether it be *soft* or *hard*, and on artificial pupil.

With regard to the *adherent* cataract, whatever be its consistence, I agree completely, and you likewise, I am sure, with Sir William Adams, as to the general principle of cutting the adherent lens into pieces, whether it be *hard* or *soft*, and then to separate it from the iris, and push the fragments of it into the anterior chamber. But as, from what I have hitherto seen in practice, the cataract *adherent* to the posterior surface of the iris is always complicated with a considerable contraction or closure of the pupil; therefore, in every case of *adherent* cataract, it appears to me to be always a better plan to make, without any exception, an artificial pupil, and, at the same time, to break the lens into pieces, whether it be hard or soft, than to abstain from the division of the iris. I mention this to you, because Sir William Adams speaks of a circumstance (p. 266) which neither I nor other practitioners in greater practice than me have ever met with; that is, of *adherent* cataract, with a pupil sufficiently dilated to allow the use of the cutting needle passed through the sclerotic coat, in order to detach and break it into pieces. What I can assert with confidence, and which you are as well, or even better, acquainted with than me, is, that the operation of the *adherent* cataract by means of the cutting or curved needle, introduced through the sclerotic coat, as in the operation of depression, is always unsuccessful, and that, on the contrary, the operation, such as is used for artificial pupil, affords a remedy for both of these diseases of the eye at the same time.

With regard to the artificial pupil, we ought to give Sir William Adams the just praise of having relieved us from the perplexity which prevailed with regard to the possibility of Cheselden's operation for the formation of a permanent pupil. In consequence of the zeal and singular dexterity of Sir William Adams, it appears that there can be no longer any dispute, whether a transverse division of the iris, to the extent of two-thirds of its diameter, be sufficient or not to leave a pupil abundantly large and permanent.

Our author gives a simple and clear explanation of the causes why this operation, such as Cheselden was supposed to have performed, did not, in the present times, answer the intention. This arose from the knife considered by the moderns as similar to that of Cheselden not having a sufficiently fine cutting edge for this delicate operation: secondly, because in withdrawing, as was the practice in the trials alluded to, the knife from the eye at one stroke, from before backwards, the iris, instead of being divided, was detached from the ciliary ligament: in the third place, because, if the transverse division of the iris, as it always happened, was less than two-thirds of the diameter of this membrane, the new pupil closed. Sir William Adams endeavoured to remove these inconveniences, by substituting for Cheselden's instrument (whatever it was) his *curved-edged iris scalpel*; and still more by not dividing the iris at one cut, but by several small and repeated strokes, as when we wish to divide *fibre after fibre*, and it had the desired effect. Notwithstanding, however, all the illustrations he has recently given on this point of practical surgery, and his having enriched, we must allow, our art with a method of making an artificial pupil, which had been considered by the moderns as useless, it still remains to be decided whether this method of Sir William Adams is to be compared with yours, which I consider as the most rational and the most useful of those which have been hitherto proposed and practised, both in respect to the facility and certainty of its performance, and its application to all the circumstances with which the closure of the pupil is complicated.

If my assertion is founded on the facts of the case, you will judge, more especially from reading the great difficulties and dangers which, as the author confesses, are met with in the performance of the operation, which he has minutely described. In this examination, and in comparing it with your method, I shall adduce no other arguments than those furnished to me by the author himself, both in his first and in his more recent work.

"A considerable degree (p. 281) of delicacy and dexterity of hand is, however, absolutely necessary for the successful execution of this operation; for, unless the iris is divided, as I have already said, as it were, fibre after fibre, it will become partially detached from the ciliary ligament; in most instances, the union of them being very slight, and very easily separated. I have found this accident occasionally occur from the merest touch of the knife. If, during the performance of the operation, the vitreous humour escapes in any quantity, (which, when fluid, it will sometimes do, notwithstanding the precaution of entering the knife in the manner recommended,) it were better for the surgeon to desist, and wait some days until the puncture has healed; long before which period the natural distention of the eye will have been restored by the regeneration of the lost fluid; for if he persist when the coats of the eye are at all flaccid, he will not succeed in dividing the iris by using a moderate pressure with the edge of the scalpel; and if he should attempt to increase the pressure, he will partially detach that membrane from the ciliary ligament, and preclude the possibility of afterwards dividing it. Indeed, when this detachment has, in any degree, taken place, it is best at once to enlarge the opening sufficiently to make an artificial pupil of a proper size, in the manner recommended by Professor Scarpa;" that is the *marginal* pupil, which is never permanent.

These difficulties, which Sir W. Adams very commendably and ingenuously has not kept concealed, are, in my opinion, of so great importance as to discourage any skilful operator, although he be possessed of that delicacy and dexterity of hand which the author requires. I confess to you that some trials on the dead subject, instead of lessening, have made me more sensible of the importance of these difficulties. The small knife, as you will have remarked, enters the eye through the sclerotic coat, at the distance of a line from the union of this coat with the cornea; from thence it is carried forwards, and, perforating the iris in the vicinity of its great margin on the temporal aspect, it enters the anterior chamber. In this position, in order that the cutting edge may be carried in a line parallel to the iris, it is necessary that its point traverse the arch of a circle within the anterior chamber, often too small for the purpose, the centre of motion of which is in the sclerotic coat, and in the point where the iris has been perforated. But as this centre in the iris is very moveable and yielding, it necessarily happens, that, in turning the knife from before backwards, to carry it in a line parallel to the iris, this membrane, at the place where it has been punctured, must be stretched from before backwards, and, therefore, readily detached at its temporal margin from the ciliary ligament. This will the more readily occur, from the point of perforation of the iris being very near to the union of this membrane with the ciliary ligament. Suppose that this first difficulty is happily overcome, it is necessary to attend particularly that the point of the knife only press slightly upon the opposite great margin of the iris, or the nasal margin, otherwise this tender membrane, too much pressed, will be detached at that part. After this it is a point of the greatest importance, the division of this soft and yielding membrane,—not at one stroke, but with small slight repeated strokes, as if you would divide *fibre after fibre*; otherwise there is a still greater risk than before of detaching it, more or less, from the ciliary ligament. And who can engage to proportion the degree of pressure to the facility with which the iris may be detached from the ciliary ligament? Besides, without a certain degree of pressure on it, it is impossible to cut it; and if this degree of pressure is too slight, the knife does not cut it. Farther, the cutting edge can only act in the manner of a saw, or backwards and forwards in a line parallel to the iris; and if it happens that the operation is too long protracted before the two-thirds of the transverse diameter of the iris can be divided, it necessarily happens, that, in the motion of the instrument in the manner of a saw, within and without the eye, the aqueous humour of the two chambers is discharged along the blade of the knife, and the eyeball collapses, and the iris is rendered still more yielding and flaccid than before. Farther, from the difficulty of making all the small repeated cuts of the knife, as in dividing *fibre after fibre*, enter one into the other, the incision of the iris proves irregular, or less than two-thirds of its transverse diameter; in consequence of which the new pupil is liable to close (p. 276) in spite of the author's precaution of interposing between the edges of the transverse incision a fragment of crystalline, in the manner of a wedge.

When the closure of the pupil is complicated with *adherent* cataract, Sir W. Adams still has a great deal to do after the transverse division of the iris, before the operation is completed, although he may have succeeded in making it to the extent of two-thirds of the diameter of this membrane. For if the nucleus of the lens, which is adherent, be so hard that it cannot be cut in pieces, after the detachment of the lens with its capsule from the iris, it is necessary to keep the nucleus in its natural situation behind the iris, in order afterwards to cut it in pieces, or push it immediately into the anterior chamber. In either case, several other operations are necessary with the double-edged needle or with the small knife; or, after piercing the eye through the sclerotic coat, and stirring about there, recourse must be had to the division of the cornea to extract the nucleus, or the entire lens, if it has not been possible to cut it down. Sir W. Adams has other difficulties to encounter besides these, if he has reason to suspect that the closure of the pupil, with adhesion of the cataract to the iris, is combined with disorganization or fusion of the vitreous humour. (p. 283.) In this case he is very careful to avoid dividing the iris at its transverse diameter, and divides it rather above this line, and then proceeds with the point of the knife to detach the capsule with the lens at its superior segment, leaving it attached to the ciliary circle at its lower segment, in order to prevent the lens with its capsule sinking, as I have pointed out to you in my preceding letter, into the bottom of the eye, *like a stone thrown into water*, and producing the most fatal consequences to the patient. In the meantime, from the uncertainty of the existence of the disorganization and fusion of the vitreous humour, the certain consequence arising from this mode of operating of Sir W. Adams, is an artificial pupil smaller than it would have been, if the iris had been divided to the extent of two-thirds of its transverse diameter, and situated at the upper part of this membrane, as it were under the upper eyelid, at the place least convenient for vision.

Compare the difficult and dangerous method of forming an artificial pupil now analyzed, with the one discovered and performed by you, of easy execution, and with equal facility applicable to the different cases of contracted and closed pupils. Having made a small incision in the cornea, you introduce your scissors closed. These do not exceed the size of a small probe; and as soon as they have entered the anterior chamber, they are in a line parallel to the iris. After this, you pass the sharp cutting blade of the scissors through the iris, the capsule of the lens, and the crystalline itself, however *hard* it may be, and with two incisions diverging from each other, without almost moving the place of the instrument, you divide all these parts at the same time, leaving a triangular flap in the iris, on the retiring of the apex of which there remains a large and permanent pupil. The iris is divided in its natural position, without its having suffered any stretching, and still less detachment from the ciliary ligament. In short, when the closure of the pupil is complicated with adhesion of the capsule to the posterior surface of the iris, or of the capsule and lens at the same time, you have every facility, through the triangular aperture of the iris, of making the fragments of either of these parts pass into the anterior chamber, where they already find an easy passage out of the eye by the incision of the cornea, through which you have introduced your scissors. Nor, in order to effect this, have you any occasion to press the eyeball, the easy spontaneous passage of the fragments of capsule, and of the lens from the posterior to the anterior chamber being sufficient for this purpose, assisted by the blunt point of the shut scissors, which answers instead of a probe, or hook, or other instrument equally simple, for freeing quickly and completely the new pupil from every opaque matter.

If farther, on the subject of the artificial pupil, according to Sir W. Adam's method, we were to take into account the success, as a proof of the superiority, you undoubtedly would not fail to produce a great number of cases besides those which are already known, and those very remarkable. But with regard to this operation, also, I wish to refrain from the comparison of the success, as an argument in favour or against a method of operating; because I consider this manner of judging as an uncertain rule. Everybody knows, that by the *lesser* and *greater* apparatus the stone has been extracted very successfully from the bladder; yet the preference is given to the *lateral* operation. There are many examples of cure of popliteal aneurism by the incision in the ham, and the ligature of the popliteal artery above and below the disease: in every case, the Hunterian is preferred to this operation. Amputation has been performed with *flaps*, but, nevertheless, Alanson's method is preferred. I therefore think, that the superiority of an operative method ought to be deduced, in the first place, from the fundamental principles of the operation, depending upon the sound and pathological state of the parts on which it is to be performed; and in the second place, from the degree of the success, which success can never fail to attend it when the principles on which it is founded are just, and the means, by which it is performed, simple and easy.—I am, &c.

LETTER V.

Pavia, 15th February 1818.

Since the last letter I addressed to you, I have had occasion to see a man having, from his birth, a very singular conformation of his eyes, such as I had never met with before. I send you the description of it, as it appears to be worthy of your notice, and because it will contribute, in my opinion, to banish more completely the fear which has been excited by Sir W. Adams, with regard to the melancholy consequences resulting from a *solid* crystalline depressed through a disorganized vitreous humour, dissolved into water.

Signor Lattuada, a young man, 25 years of age, an inhabitant of Rosali, in the Milanese, near-sighted from his infancy, was affected with a constant rotatory motion of his eyes, similar to persons born blind. As he grew up, he experienced great difficulty in learning to read and write, on account of his eyes always filling with immense quantities of tears; his head was affected with tremor, and his eyes rolled about with more rapid movements than before. This imperfection was considered to be simply spasmodic; to cure it, he was ordered anodynes internally, and narcotics externally; such as the leaves and extract of belladonna, but without any benefit, and, if we can believe the account of the patient, with disadvantage.

The following is the appearance of the right eye:—The cornea is rather more prominent than that of the other eye. There is not the smallest vestige of iris,—a most singular appearance. The lens, inclosed in its capsule, and opaque, particularly at its centre, is loose and free from all attachment to the vitreous humour, and is perceived moving in the cavities of the eye in various directions; sometimes with great rapidity, according to the quicker or slower motion of the ball of the eye, of the palpebræ, or head. When the young man bends his head downwards, the lens inclosed in its capsule advances nearly, and sometimes completely, into contact with the cornea. When he bends his head backwards, this whitish body is perceived distinctly falling down into the bottom of the eye, in a plane inclined from before backwards. In the perpendicular position of the head, a segment of the lens sometimes presents, sometimes it disappears anew, as I have mentioned, according to the motion of the eyeball, and more or less strong and rapid movements of the eyelids. This opaque body never stops in the axis of vision, and, therefore, it never completely intercepts the passage of light to the back of the eye. When the head is bent backwards, the lens evidently descends to the bottom of the eye, with such facility as it would do in a thin and watery fluid. During the time that the lens is kept seated at the bottom of the eye, the young man says that he sees a dark yellow body, with black points on it. If he remains long in the horizontal posture, as during the night, and, consequently, with the lens resting on the bottom of the eye, he has never experienced the slightest uneasiness from the pressure of the lens on the retina, and he has never been affected with pain or inflammation of the eyes.

In the left eye the derangement is less than in the right. In the left, there is a small strip of fringed-shaped iris at the temporal angle. The lens, with its opaque capsule affected with cataract, is, as it were, suspended in its place by a short attachment, which it has preserved with the *ciliary zone* at the upper and temporal part; a proof, it would appear, that in this eye, the vitreous humour is not entirely disorganized and dissolved into water. There is no attachment whatever of the rest of the circumference of the capsule or lens, and during the motion of the eyeball and head it oscillates a little from before backwards, preserving its natural situation from the small portion of it which is united to the *ciliary zone*, at the upper and temporal side. The young man sees all objects tolerably well, but he distinguishes those best which are at a short distance from him. On making him try a very convex glass, a little stronger than is employed by patients who have been operated on for cataract, he distinguishes minute objects very well, and they appear larger than they naturally are. On placing before his eyes a mask, having at a proper distance, two small holes corresponding to the pupil, if there had been one, such as is employed for curing the *strabismus* of children, with the view that he should receive the light as it were through the natural pupil rather contracted, he distinguished minute objects with greater accuracy than before, such as the small letters of a book, and much better than without this assistance, and further they appeared larger than they actually were, as if he had been using the very convex lens. A different result was obtained on repeating the same experiment with the left eye, with which, although the mask was accurately applied, so that the small hole corresponded with the axis of vision of this eye, he could not see any at all. The cause of this was, that the ray of light which entered the small hole, fell precisely on the opaque lens, while, on removing the mask, the light, from the total want of iris, passed to the bottom of the eye on the nasal and lower side of the opaque lens, suspended at the upper and temporal angle.

To this fact, which there is every reason to suppose merely the effect of malconformation, I add the following reflections. Among the many suppositions made by Sir William Adams with regard to the frequent complication, as he thinks, of cataract with disorganization of the vitreous humour, is this, that the opaque lens with its capsule may remain in its natural situation, notwithstanding the conversion of the vitreous humour into water. In making this assertion, he has not only deceived himself, but has likewise committed a gross error in anatomy. Whenever the vitreous humour is completely disorganized and fluid, as in the case just related, the lens with its capsule cannot possibly retain its natural situation unless there be some other morbid affection, in consequence of which the capsule has contracted adhesions to the ciliary processes, or to the posterior surface of the iris. The error into which Sir William Adams has fallen, has arisen from his having supposed that the capsule of the crystalline adhered, in the healthy state, to the *ciliary processes* ; which anatomists know to be notoriously false. The capsule of the crystalline has no other union but with the *ciliary zone*, under which *zone* is the canal of Petit ; this is as much as to say, that the lens, with its capsule, has no other natural connection but with the membrane of the vitreous humour, which passes over the greater convexity of the capsule. Now, if the crystalline with its capsule has no other attachment but to the vitreous humour, whenever this humour becomes disorganized and converted into water, the membrane investing it necessarily collapses, is torn into several pieces or disappears, the crystalline with its capsule being deprived of the point of support and union posteriorly, ought of necessity to quit its natural position, and appear as a free body floating within the eye, precisely as in the case which I have just described. On the contrary, Sir William Adams, in the persuasion that the lens with its capsule was in the sound state attached to the *ciliary processes*, deduced as a consequence, that the lens, notwithstanding the fusion of the vitreous humour, might remain in its situation. And this inference surprises me the more, that this author, in another place, (p. 106,) says, that the capsule with the lens is easily detached in similar circumstances from the ciliary processes, on account of its being deprived of the *tunica aranea* from a disease of the vitreous humour, which tunic, in its natural state, retains the capsule with the lens firmly in its situation. "From the superior gravity of the opaque lens, it therefore sinks immediately to the bottom of the eye, on being detached from the ciliary processes, like a stone thrown into water, there being no *tunica aranea* to assist, as is naturally the case, in securing it in its situation."

After this reflection, how could Sir William Adams persuade himself, and persons acquainted with anatomy, that a case of cataract could occur which would remain in its natural situation in spite of the disorganization of the vitreous humour? And how could he ever add, that, in these cases, on the slightest touch with the needle, the lens, with its capsule, quits its attachment to the *ciliary processes*, and is precipitated to the bottom of the eye like *a stone thrown into water;* while, in direct opposition to this, when there is a disorganization of the vitreous humour, the lens, with its capsule, is free from every attachment and floating within the eye? How could he repeatedly, on this erroneous foundation of a cataract remaining in its situation, although slightly attached to the *ciliary processes*, establish the diagnosis of cataract complicated with disorganization and fusion into water of the vitreous humour? But this ready separation of the capsule from its natural connections, which indeed is sometimes met with in the practice of the operation of depressing the cataract, may equally occur, although the vitreous humour is in a perfectly healthy state; because this easy detachment may take place whenever the portion of the *tunica aranea* which passes over the capsule of the crystalline is unusually fine, and, therefore, easily lacerable by the pressure of the needle on the solid crystalline. Richter (Fasc. II. p. 96) mentioned that he had operated on four patients by extraction, in whom the cataract *passed out inclosed in its capsule*, without there being the slightest appearance in these patients of disorganization or fusion of the vitreous humour; which dissolution, if it had existed, would have undoubtedly manifested itself after an incision of the cornea, which must have been large, to allow the easy exit of the lens surrounded by its capsule, and, consequently, more bulky than usual.

Reflecting for a moment on the case which I have related, what would you say to any other expert oculist with regard to Sir William Adams's so much dreaded melancholy consequences of a *solid* crystalline depressed into a vitreous humour converted into water? According to his opinion, the *hard* depressed crystalline never fails to destroy the soft retina in the midst of most excruciating pain and most violent inflammation of the eye. And yet, in the case above related, none of these consequences have occurred in the course of 25 years, nor is this any reason to expect that they will supervene. If, farther, the phenomenon were constant, (which I neither pretend to assert nor to deny,) that the lens depressed, in such circumstances, moves in correspondence with the motions of the eyeball, of the eyelids, and head, it appears probable to me, that this phenomenon, sometimes at least, would have been observed in the numerous cases of easy detachment and of quick depression of the *solid* crystalline, in which cases Sir William Adams supposes that the fusion of the vitreous humour is always conjoined with the cataract. But no operator has hitherto mentioned this phenomenon, consequent to depression, with easy detachment of the whole capsule with the lens from the *ciliary circle*. This mode of reasoning of mine is founded, as I have mentioned to you, on the fact above related. Nevertheless, of, after reading this letter, it should occur to you to observe to me, that as Sir William Adams, from not having dissected the eyes of the 14 Greenwich pensioners, was not warranted in asserting that in them the vitreous humour was wholly or partially disorganized and converted into water, in like manner, that I, from not having dissected the eyes of *Lattuada*, am not authorized to say that the vitreous humour was completely converted into water in the right eye, and partially in the left; I shall merely reply, that you will at least acknowledge yourself indebted to me for this rare and singular case, and inform me how it happens in this case, that if the vitreous humour were healthy, consistent, and globular, the crystalline inclosed in its capsule, quite detached in the right eye, can be moved in every direction within the ball of the eye, and still farther, evidently fall from before to the bottom of the eye, whenever the young man bends his head backwards. After the rupture of its external tunic, the crystalline free, floating, when not pushed forcibly into the cavernous texture of the healthy and consistent vitreous humour, never passes backwards, in consequence of its gravitating power, farther than the posterior chamber of the aqueous humour; and if it sometimes rises or sinks during the motions of the eyeball and lids, it does so in a direction parallel to the posterior surface of the iris, and sometimes threatens to pass into the anterior chamber, but never passes from the anterior chamber to lodge in the bottom of the eye.—I am, with the greatest esteem and friendship.

VII.

A Letter to the Right Honourable Lord Viscount Palmerstone, Secretary at War, &c. &c. on the subject of the Ophthalmic Institution, for the Cure of Chelsea Pensioners. By JOHN VETCH, M.D. F.R.S.E., Physician to the Forces, Member of the Medical and Chirurgical Society of London, and of the Royal Medical Society of Edinburgh. 4to, pp. 10. London, 1819.

WHEN the preceding article was at press, we received Dr Vetch's letter, and we postpone other matter to make room for some account of it. We are anxious to use the means in our power to make it quickly and generally known, and we shall deliver our sentiments upon its merits with perfect independence, and, we trust, without prejudice. To our examination of the question we come without any political or personal enmity to the noble lord to whom it is addressed. We have always considered political discussion as inadmissible into a professional work, and towards Lord Palmerstone our feelings are neutral, but, on the present occasion, he has been misled to commit what is in our opinion a great wrong;

"*Amicus Plato, sed magis amica veritas.*"

It is well known that our troops imported with them from Egypt the contagious ophthalmia, and that it rapidly spread not only through the army, but even threatened like a pestilence to spread over all the land. Unexampled in its severity, and unknown in its nature, it resisted the means at first employed against it, and the nation witnessed with regret the sufferings of its warriors of whose services it deprived them. As the duties of the regimental surgeons did not permit them to direct their whole attention to this disease, important as it was, and for other equally solid reasons, it was thought advisable to form depots for soldiers affected with ophthalmia, and barracks at Selsea, Aldwick, and Bognor, were allotted for this purpose. Dr Vetch was appointed to their medical and economical superintendence.

About the time that Dr Vetch was occupied in the ophthalmic depot, the attention of the surgeons in civil life was directed to it, and it naturally attracted the particular notice of the late Mr Saunders, who had recently founded an institution in London for curing diseases of the eye, and afterwards of his pupil Mr Adams, now Sir William, who had succeeded him at Exeter. This individual was fortunate enough to attract the notice of some persons of rank in the army, and he had abilities enough to take every advantage of it. He came to London with the view of succeeding Mr Saunders in his practice, and advertised that he alone was fully acquainted with all the secrets of his deceased instructor. Mr Adams also boldly asserted, that he had made several discoveries relating to the Egyptian ophthalmia, and he had interest enough to prevail upon the Directors of Greenwich Hospital and of St Pancras Work-house to allow him to make trials of his skill. Misled by the apparently successful results of these trials, and the boastings of Mr Adams, some individuals in power took him under their protection. He was created a knight by the Regent, and the Minister at War instituted a new ophthalmic branch at York Hospital for soldiers, at the head of which he placed Sir William; and, we learn, there is now building a separate hospital, to which it is the avowed intention of Lord Palmerstone to appoint Sir William.

On the 11th of May 1819, our countryman, J P. Grant, Esq. moved in the House of Commons for certain papers connected with the establishment. His motion was successfully resisted by Lord Palmerstone, and the observations then made by the Secretary at War have induced Dr Vetch to address to him the unanswerable letter now lying before us.

Mr Grant's motion chiefly related to the want of economy and extravagance in forming a new and expensive establishment at such a time, and that too, not with an army, but a private, practitioner at its head. On the former branch of his inquiry we shall only remark, that the expediency or inexpediency of the establishment depends upon the existence or non-existence in the army of the disease it is intended to counteract. It is alleged by the army surgeons that the disease is eradicated; while Lord Palmerstone asserts, that at this very time, whole regiments are labouring under a most severe inflammation of the eyes. From our personal knowledge, we believe his Lordship to have been misinformed. But if there be even any grounds for suspecting that *contagious* ophthalmia is not eradicated, we think that the noble secretary deserves praise for devising any means of opposing its progress and ravages.

Admitting the expediency of erecting an ophthalmic hospital to be established, we have no hesitation in stating it as our firm and conscientious opinion, that it should have been placed under the charge of a medical officer of the army; and, we must confess, that we read with regret the newspaper reports of the manner in which their services, abilities, and moral character, were spoken of by the Secretary at War. Newspaper reports are not always correct, and we hope that, in this instance, they are altogether erroneous. It could never be intended to degrade, in the opinion of the public and of the army, a body of men who deserve well of their country for the able discharge of their professional duty, and whose integrity and high sense of honour command universal esteem. Irregular promotion is an injury to every branch of public service, but most especially to those where, as in the medical department of the army, much depends upon a knowledge of detail business, only to be acquired by experience. The certainty of promotion, if they deserve it, is a strong inducement to the zealous performance of inferior duties, while irregular promotion always creates disgust, and gives offence not only to those unjustly passed over, but to all who have experienced advantage from their services, as, we well know, the whole army has from their surgeons. There are, we admit, occasions which warrant and even demand a deviation from routine promotion, but the case must be urgent, and indisputably made out. We have now to inquire whether the present be such a case. A new ophthalmic hospital is preparing.

According to the rules of service it should be officered by the regular medical department of the army, so many of whom are now turned off to starve on half-pay. But it appears that an individual is to stand at the head of this establishment who never was in the army, who never partook of the hardships and dangers of service, and who is not subject to military discipline or to military responsibility. We know not whether the etiquette of the army will permit junior officers to serve under one who does not bear his majesty's commission, but this is comparatively of little consequence, and we have no desire to excite mutiny. But officers and soldiers might fairly argue from it that their health was submitted to the care of incompetent persons. The real point at issue is, Whether there is no medical officer in the army sufficiently skilful to be entrusted with the management of the new ophthalmic hospital? or, Whether Sir W. Adams is so much more skilful than any of them that it is expedient to violate the just rules of service? Upon a point of this kind we conceive ourselves to be much better judges than the Right Honourable Secretary; and we have no hesitation in stating, without fear of contradiction by any one qualified to give an opinion, that there are hundreds of medical officers in the army fully equal to the service, and that Sir W. Adams is by no means so well qualified as many of them.

To bring the question within a narrow compass, we shall contrast what Dr Vetch has said of himself and of Sir W. Adams.

> " Having been the first to ascertain and to establish that the infection of Ophthalmia was exclusively conveyed by the contact of the discharge from the diseased eye, and that the healthy state of the lining of the eyelids was the indispensable criterion of the recovery from the danger of relapse, and from the power of communicating the disease to others—I am indisputably entitled to whatever merit may attach to the early adoption of these conclusions, the truth of which I can support by consecutive evidence from the year 1806 to the present time.
>
> " Secondly, When upwards of 4000 men had been lost to the service in other places, by this painful and afflicting disease—out of 3000 who were treated under my immediate care at the Ophthalmic Hospitals of Selsea, Aldwick, and Bognor, not one man admitted in an early stage of the disease was disabled from military service; while from 500 to 600 cases of blindness were recovered and rendered fit for active or garrison duty, as far as depended on the state of their vision: thus effecting an immediate alleviation of human suffering, and a saving to the country, which, by the average loss at other places, cannot be estimated at less than L. 20,000 per annum.
>
> " In addition to this result of my immediate and personal treatment; by my official correspondence with the surgeons of various corps,

and by printed tracts, I materially contributed to the amelioration and eradication of the disease in other places; and, while so much was effected by medical treatment, the whole expense was not only defrayed out of the usual stoppages deducted from the pay of the soldier when in hospital, but an actual and available surplus of L. 2000 carried over to the public account.

"Thirdly, So fully had I considered the various aspects of the disease, both as it regarded the army and the nation at large, that while I took especial care, so far as my control extended, that no man should be discharged on account of blindness in whom there was any vestige of remaining disease, or any prospect of ultimate recovery,—I addressed a representation on the subject so far back as the beginning of 1808, through Lieut.-General Lord Charles Somerset, and which I repeated from time to time, on the mischievous effects of sending blind men to their homes, instead of detaining them for farther treatment; a precaution which, if then adopted, would have saved the doubtful and expensive policy on which your Lordship is now acting, of calling these persons from their homes at so distant a period, and when, from the recently printed statement of Sir William Adams, it appears that only one man has been admitted in an infectious state of the disease." P. 4, 5.

"About the same time, viz. in 1806, though unknown to Mr Saunders, I had fully ascertained and demonstrated in my practice, the importance to be attached to this stage of the complaint, and the necessity of a strict attention to the internal surface of the eye-lids, as the only and indispensable criterion of recovery. I found by farther experience, that various caustic and astringent substances, carefully applied, were superior in efficacy to the use either of the scissors or the knife; the truth of which statements I am ready to prove to your Lordship, by the personal and written testimony of those who assisted me in carrying the practice into execution at the time.

"After all that has been said and written respecting this affection, as connected with Opaque Cornea, I am satisfied that many circumstances necessary to give effect to the treatment I have recommended, are not sufficiently understood, or attended to; and the superiority of my success I still am, as I have always professed myself to be, willing to prove, by a comparative trial with Sir William Adams, or with any other person; I totally and distinctly deny that I ever received any information whatever from the practice or publications of Sir William Adams, my own having been fully matured before he was known to the public." P. 2, 3.

"Besides the various printed misrepresentations, which have been circulated, with incredible industry, among all whose influence has been deemed essential to the interest of Sir William Adams, I find that to certain copies he has ventured to affix a *manuscript* note, with the grave assertion that the loss of men in the 52d regiment of foot, which I have detailed in an account of Ophthalmia published in 1807, took place under my immediate care and treatment. A result which he desires the reader to contrast with his treatment of the children of St Pancras Work-house. For the correct information of your Lordship, and those who may have given credit to this statement, I am alone induced to notice it, and to say that, from my publication of 1807, quoted by Sir William Adams, he knows as well as I do, that *when the great loss occurred in the 52d regiment, I had not even seen the disease in question; that I never had the medical treatment of that regiment;* and that I only arrived in time to observe the effect of the treatment, which instantly arrested the destructive progress of the disease.—A treatment which, as afterwards matured, has hardly been equalled, for the certainty of its success, in the history of any disease whatever." P. 6.

Although we give implicit credit to Dr Vetch's assertions, which he durst not have made if they were not true, yet he brings the highest testimony to confirm his claims. Mr Knight, Inspector-General of Army Hospitals, at an early period of his services, officially reported his opinion of them to the private secretary of the Commander-in-Chief, in August 1808.

> "In the superintendence of these concerns, I had great reason to be pleased with the skill, arrangement, and extreme attention of Dr Vetch; and I should ill discharge my duty, if I did not report the same to His Royal Highness the Commander-in-Chief.—Indeed, he seems to be so well qualified for this peculiar duty, that I cannot avoid recommending him for the appointment of Staff Surgeon to the Forces, to be specially attached to this duty; for it appears that he has been remarkably successful: and of the many cases sent to Selsea, very few have lost their sight at that place.—It is much to be regretted, that more Hospital Mates cannot be sent to him, for he is in great want of such assistance; but I have been repeatedly obliged to recall them from this duty, for the pressing exigencies of foreign service, and have not been able to replace them temporarily from civil life." P. 7.

The present Director-General is equally favourable in his opinion, officially transmitted in April 1810.

> "Dr Vetch deserves no less credit for his successful aud scientific treatment of ophthalmia, and the various diseases of the eye, than for his economical and proper management of the different stations into which the depôt is divided. I hope soon to be able to present you with specific returns on these heads. I will at present only state, that of 800 admissions during the last year, in only three cases has vision been lost, and in these cases, which were shewn to me, Dr Vetch is not without hopes of sight being recovered. At this depôt many of the most delicate operations have been performed on the soldier, with a degree of success which does not always fall to the lot of the first oculists.
>
> "When every expence is included at this establishment, the total is rather under 9*d*. a day per man, under the disadvantage of the three

stations being separate, the markets being nine miles distant, and when hospital servants, the purchase and repair of boats, wages of watermen, &c. are included." P. 8.

"Since the establishment of the depôt in this neighbourhood, I have had the satisfaction to see a very considerable number of men discharged cured to their regiments, or sent for duty to veteran battalions, who were sent blind to the depôt; for this, the service is indebted to the skill and zealous exertions of Dr Vetch." P. 9.

We should next inquire upon what grounds Sir William is to be preferred to so meritorious an officer, but we fear to misstate them. We believe that he pretends to have discovered a better mode of treating the disease in its early stage, by violent vomiting. His teacher, Mr Saunders, treated it by exciting nausea, on the rational principle of diminishing arterial action. But Sir William, to prove his originality, contends that his vomiting operates upon the opposite principle of exciting a new action. We shall not stop to expose the absurdity of the principle, since the practice is so decidedly bad that it is not followed by a single individual, and is not continued, so far as we believe, by Sir William himself. So much for his superior treatment of the first stage.

We have next to examine Sir William's pretensions to the discovery of a new and successful mode of treating the second stage.

"It is no longer a question that Sir William Adams was taught the knowledge of this stage of the disease, and its treatment by excision, by the late Mr Saunders; in return for instruction so liberally given on this and other occasions, he sought, by substituting a knife in place of the scissors used by Mr Saunders, to appropriate to himself the whole merit of the discovery. The change of the instrument was, even if successful, of very inferior moment; and, unfortunately for such pretensions, has, after a trial much too long and perseveringly adhered to, been found to be so objectionable, as at length to have been laid aside in practice, even by Sir William Adams." P. 2.

These quotations contradict all Sir William's pretensions to originality in the discovery of a method of treatment which has stood the test of experience. The success of his actual treatment is next to be ascertained. When Dr Vetch left the Ophthalmic Depot, he returned 127 men in it as in a state of recovery for garrison duty.

"From this class Sir William Adams, in the presence of Dr Franklin, Deputy Inspector Albert, and Mr Usher, selected five cases after my removal,—that they were not considered incurable by me must appear from the head under which they were regularly returned to the Army Medical Board; and that they were cured by Sir W. Adams remains to be proved." P. 10.

The patients since placed under Sir William's care were not admitted indiscriminately as they occurred, but were selected cases. We have the pleasure of knowing the gentleman who came to this country to select and send up cases to be treated by Sir William. We even recommended some to him ourselves, who were refused, because " he did not think himself justified in putting the country to expence, where he did not see hopes of success." Granting that Sir William had succeeded in improving the vision of several patients thus selected, it would not have furnished any conclusive evidence of the superiority of his practice. It is upon such proof that the reputation of all quacks is established. They cure or relieve a few cases, deemed incurable by regular practitioners, which they advertise in proof of their superior skill, while they are not censured for their numerous failures in cases which might have recovered under more scientific treatment We have not yet forgotten the confidence with which many well-meaning people received Delahoydes's pretensions to cure mental derangement. This person, also, apparently succeeded with lunatics selected from the government asylums, and deemed incurable; and he contrived to get a report from a respectable committee favourable to his views, (See Edinburgh Medical Journal, Vol. X p. 250.) Mr Delahoydes's imposture, however, did not thrive in England, and his career in Ireland was soon terminated by a public trial and conviction.

" To come, however, to what more immediately must interest the public, the actual result of the cases which have been placed under the discretionary treatment of Sir William Adams.

" Against the admission of the printed report drawn up by Sir William Adams, of the cases he has treated during the last year, as a verified or authentic document, until compared with the evidence of the patients, and of the two Boards, at present withheld, I must be allowed strongly to protest.—

" That the operations of Sir William Adams, as stated by himself, are less successful than those of others whose veracity is unimpeached, may be seen by a reference to the practice, and annual reports of the Cork Street and City of London Infirmaries, for the cure of diseases of the eye.—While, with respect to the treatment of Opaque Cornea, the success has been infinitely less than the result obtained under my care and direction, though conducted with every local disadvantage, while his practice has been aided by the immediate and official patronage of your Lordship.

" The cases of operation, admitting the statement given, of the patient's vision, to be correct, produce in those competent to give an opinion, one very different from what a superficial view may have conveyed to your Lordship and others, who, with all deference, cannot be supposed capable of entering fully into the merits of such a question. Most of these confer any thing but credit on the operator, being cases where, with the total loss of one eye, a greater or less degree of useful vision existed in the other: in all of which, I assert that the hazard of attempting an operation, as was done, upon the only remaining eye, was wholly unwarranted by any benefit which, from the state of the organ, *it was even possible for the operation to bestow;* while the risk and consequences of a failure were such as never could have been hazarded in general practice, or consented to by the patient, if the chances were fairly submitted to his choice; the operation was undertaken in contradiction to sound judgment, and to any humane feeling for the patient. In support of what I assert, I may appeal to the opinion and practice of any experienced surgeon, or oculist, throughout Europe; but to offer a practical comment, it only remains to be told, that out of the limited number of patients treated at so great a charge to the country, and with such informality to the service, *no less than five individuals, after being brought a distance exceeding* 400 *miles, and admitted with partial and useful vision of one eye, have been consigned to irremediable blindness through the failure of an injudicious operation,* and have either received or are now applying for, an increased rate of pension for the injury they have sustained.' P. 3, 4.

But the testimonies of Sir H. Halford, Dr Baillie, Mr A. Cooper, Mr Abernethy, and others, have been quoted in favour of Sir William. These testimonies were given in the year 1814, related to a single pretended improvement, and proceeded upon a very limited experience. We should be satisfied to refer the whole question at issue to the deliberate opinions which the same gentlemen would give in 1819. It appears, indeed, from Dr Vetch's letter, that the Secretary at War very properly took measures to get the opinion of persons, whom, we must suppose, he then thought competent judges.

"When, however, the moment has arrived for this investigation, we are informed by your Lordship, that the Reports made by the two Boards specially appointed to observe and record the evidence on this subject, having been dictated by a spirit of professional jealousy and hostile combination against Sir William Adams, are unfit to be produced; and that your Lordship's own testimony, and the statement given by Sir William Adams himself, render further evidence not only unnecessary, but inadmissible." P. 1.

After these observations, our readers will be at no loss to conceive why the Secretary at War should resist Mr Grant's motion, for copies of all communications which passed between the Commander-in-Chief, the Medical Board, and the Secretary at War, respecting the establishment for the cure of ophthalmic complaints, and for several other reports and opinions of different medical officers on the subject.

PART III.

MEDICAL INTELLIGENCE.

REPORT of DISEASES treated at the New Town Dispensary, *for the Quarter ending May 31st* 1819.

Febris continua	203	Hæmoptysis et Phthisis	35
——— ephemera	17	Dyspnœa	5
——— infantum remitt.	38	Asthma	6
——— dentitionis	18	Palpitatio	4
Hydrocephalus	4	Pertussis	46
Cephalæa et vertigo	42	Hepatitis	10
Apoplexia	1	Icterus	2
Paralysis	7	Dyspepsia et Hypochondriasis	53
Epilepsia	4	Hysteria	15
Convulsio	2	Vomitus	3
Ophthalmia et sequelæ	59	Hæmatemesis	1
Cataracta	1	Enteritis	2
Amaurosis	1	Gastritis	2
Otalgia	2	Colica et Obstipatio	14
Epistaxis	2	——— Pictonum	1
Odontalgia	20	Diarrhœa	37
Aphthæ	3	Cholera	1
Dysphagia	1	Dysenteria	2
Cynanche tonsillaris	18	Vermes	21
——— maligna	1	Tabes mesenterica	4
——— laryngæa	1	Hæmorrhois	12
——— parotidœa	1	Hernia	5
Catarrhus	125	Prolapsis uteri	1
Pneumonia	48	Amenorrhœa	15
Peripneumonia	1	Dysmenorrhœa	4
Pleurodyne	52	Menorrhagia	4
Pericarditis	1	Leucorrhœa	8

Dysuria	1	Strophulus	1
Nephralgia	2	Lepra	4
Hydrocele	1	Psoriasis	6
Gonorrhœa	4	Pityriasis	1
Inflammatio testis	1	Ecthyma	2
Hydrops	22	Intertrigo	4
Physconia	1	Eczema	1
Rheumatismus	35	Acne	2
Lumbago	9	Impetigo	13
Scrofula	22	Rupia	1
Rachitis	1	Roseola	2
Morbus spinalis	1	Urticaria	3
Syphilis et Pseudo-syphilis	16	Porrigo	15
Phlegmon	26	Scabies	53
Anthrax	1	Herpes	13
Mastodynia	9	Tumor	1
Paronychia	3	Ulcus	25
Erysipelas et Erythema	7	Vulnus	5
Pernio	1	Ustio	11
Variola	17	Luxatio	2
——— modificata	3	Contusio et subluxatio	53
Varicella	1	Venæ varicosæ	1
Rubeola	64	Asthenia	7
Scarlatina	15		

An uncommonly mild winter yielded in a gradual manner to spring, which has now passed into summer by almost insensible degrees. The weather has been unusually temperate, vegetation has rapidly advanced, and is a month earlier than in last season ; but, during the last fortnight of May, east winds were very prevalent, and were as usual accompanied with a cold and moist atmosphere.

From the list of diseases, it appears that the cases of fever have been as numerous in this as in the preceding quarter ; but there can be no doubt that the epidemic is rapidly on the decline, and of the 203 cases mentioned, less than a fourth have occurred in the last month. It is to be hoped that the favourable season, and the improving circumstances of the lower orders, will put a stop to this disease, which has brought upon the poor such accumulated suffering.

The quarter has been chiefly distinguished by the occurrence of an epidemic catarrh or influenza. It will be observed, 125 cases of catarrh stand on our list, while there are only 15 in the preceding quarter. What resemblance it bears to the epidemics of 1803, and former years, we have not ascertained ; it certainly appears to be much milder, less generally prevalent, and of a shorter duration. It is even now on the decline, after having lasted only a few weeks. It has the ordinary symptoms of pyrexia, generally, but not invariably, joined with catarrh. In some instances it has been with difficulty distinguished from continued fever, but in most cases the symptoms are slighter, the tongue is less heavily furred, the sensorial functions are less disturbed, and there has been but little pain in the back. It is contagious, at least many members of the same family have been afflicted with it. Its duration is usually very short, not exceeding two or three days; and a crisis almost universally takes place with sweating. The simplest remedies have been used with success, and these have very frequently been domestic diaphoretics. Bleeding and other active remedies have seldom been required, but in several cases in which the disease had been neglected, loss of blood seemed indispensable. Considerable debility and mental depression have accompanied this disease, and made the convalescence frequently very slow, thus easily distinguishing it from common catarrh or from ephemera.

Account of the tying of the Subclavian Artery, by M. Dupuytren. Communicated by Andrew Combe, Esq. Surgeon.

THIS day Dupuytren, surgeon of this hospital, tied the subclavian artery of the left side, just as it emerges from behind the anterior scalenus, in a case of false aneurism of the axillary artery, in a man of thirty-seven years, of an excellent and robust constitution. He was a soldier, and seven years ago, in a charge of cavalry, received a thrust of a sword from behind, which opened the axillary artery. He lost much blood, but the flow was stopped by the dressing, and the wound healed in three weeks. A false aneurism gradually came on, and has continued to increase, slowly at first, but for some time past more quickly. The tumour now distends the axilla enormously, distends the pectoral, raises up the clavicle, and is about the size of a child's head. The skin is not at all discoloured. The pulsation in the radial artery is with difficulty perceptible. The arm preserves its natural heat, but is a little benumbed. He is by trade a house-carpenter, and has continued at work till about six weeks ago, when he could continue no longer. He has been in the hospital about a fortnight, and has been kept tranquil, but no application made. He was operated upon in bed to avoid the annoyance caused by the noise of those in the amphitheatre who do not see. Dupuytren made his incision about three-fourths of an inch above the clavicle, beginning close at the outer edge of the sterno-mastoid, a small part of which was cut. He continued the incision outwards and downwards about two inches, so as to come nearer the clavicle. By this incision the greatest convexity of the clavicle was nearer the inner end of the wound than the outer. He proceeded with extreme caution, separating the cellular substance, filling up the space between the sterno-mastoid and trapezius with the handle of the knife, or the end of his scissors, in order to spare the larger branches which go down from the cervical nerves to the thorax. Having isolated the artery from the nerves, he tied the ligature round it, and brought the lips together. The patient was bled, and kept on low diet. He passed a good day and night, and complained of nothing. The operation lasted one hour and a quarter, but he was extremely cautious. The pulsation in the radial artery was stopped. No inflammation took place. Every thing went on well, and about the sixth day the tumour began sensibly to diminish. About the fifteenth day the ligature came away; and on the 27th of March, when I last saw the wound, it was nearly healed. A little pus came from it on one point, on pressure. The tumour is diminished about one-third, and is still diminishing. The arm preserved its temperature. Thus there is every prospect of cure, the patient having now passed twenty days well since the operation. A detailed account of the operation will likely be published by Dupuytren himself in the French Journals, as soon as the ultimate result is known, so that it is unnecessary to say more here.

Hotel Dieu, Paris, 7th March 1819.

Cure of the Cholera Morbus of India, extracted from a Bengal Newspaper.

SIR,—Having, on a former occasion, requested the insertion in your paper of my opinion relative to the most effectual treatment of cholera morbus, I have to request you will give publicity to the following case, which has fallen under my observation. I was requested to visit a bearer, who, three hours previously, had been attacked with cholera morbus; there was incessant vomiting and purging, his extremities were cold, as also the skin, the pulse could not be distinguished. I, of course, entertained slight hopes of his recovery, but gave him immediately forty grains of calomel, followed by laudanum one hundred drops, twenty drops of oil of peppermint, in four ounces of brandy, and ordered the abdomen to be constantly rubbed with warm brandy. The medicine remained on his stomach for three quarters of an hour, when the vomiting returned; he vomited a worm of eight inches in length, but without any alleviation of the sickness at stomach. He had now eighty drops of laudanum, twenty of oil of peppermint, and three ounces of brandy, as also twenty grains of calomel, after which he rapidly recovered without the slightest inconvenience from the calomel. I feel so perfectly convinced of this summary treatment affording the most likely hope of recovery, that I am solicitous you would do me the favour to insert it in your paper. Your obedient servant,

22d March. CHIRURGUS.

THE Anniversary Dinner of the Members of the "Society for the Benefit of the Widows of the Officers of the Hospital and Regimental Medical Staff of his Majesty's Army" took place on the 26th of May, at the Thatched House Tavern, St James's Street, Dr Franklin, Principal Inspector of Hospitals, in the chair.

Eighty gentlemen sat down to dinner, and never did a more harmonious or happy party assemble. Besides Sir Everard Home, Sir Matthew Tierney, Dr Somerville, Mr P. Macgrigor, Surgeon of the Lock Hospital, Mr Guthrie, and several other medical officers of th army, the Society was honoured with the company of Dr Baillie, Sir Gilbert Blane, Dr Godden Jones, Dr Copeland Hutchison, Mr Wilson, Mr Brooks, Professor Rennett of Cambridge, Mr Brodie, Dr Wilson, Mr A. Todd Thomson Mr Alexander, and several others of the principal medical characters of the metropolis.

Dr Baillie, Sir E. Home, Messrs Wilson, Thomson, Hutchison, Alexander, and the Rev. Mr Clarke, the chaplain, severally addressed the meeting in eloquent, manly, and appropriate speeches.

We feel sincere pleasure in having it in our power to state, that the funds of this excellent Institution are in the most flourishing condition. It was founded by Sir James M'Grigor, in 1816, and is supported solely by the subscription of medical officers of the army. Its members now amount to 585, and they possess an accumulated sum of upwards of L.13,000 sterling.

The following Gentlemen have been appointed Presidents and Stewards for the ensuing meeting:

G. DICKSON, Esq. in the Chair.

Sir Matthew Tierney, Sir James M'Grigor, W. James, Esq. J. Hodges, Esq. M. Barmester, Esq. S. Reed, Esq. J. A. Guthrie, Esq Dr D. Gregory, Dr Emerson, W. Lewis, Esq. P. M'Grigor, Esq. Dr Thomson.

A SOCIETY has been established in London bearing the designation of the "Hunterian Society." It professes the most friendly feeling toward all similar existing institutions, and is founded principally, but not exclusively, for the accommodation and benefit of medical men residing in the eastern parts of the metropolis.

Its objects are, to concentrate the zeal and experience of a large number of respectable practitioners, whose places of residence are at a distance from professional associations already existing, and to receive and discuss communications on medical and surgical subjects. It aims particularly at the cultivation of a spirit of liberal and friendly intercourse among the members of the profession within the sphere of its influence.

It consists of honorary, corresponding, and ordinary members, and already the society is honoured by the names of a considerable number of men of character and talent. The following is the list of the officers and council for the present year.

President, Sir William Blizard, F. R. S.—*Vice-Presidents*, James Hamilton, M. D George Vaux, Esq John Meyer, M. D. Lewis Leese, Esq.—*Treasurer*, Benjamin Robinson, M. D.—*Secretaries*, John T. Conquest, M. D. F. L. S. Thomas J Armiger, Esq.—*Council*, Thomas Addison, M. D. Thomas Bell, Esq. F. L. S. Henry James Cholmley, M. D. Thomas Calloway Esq. William Cooke, Esq. George Edwards, Esq. James Alexander Gordon, M. D. William Kingdon, Esq. Benjamin Pierce, M. D. James Parkinson, Esq. Henry Richard Salmon, Esq. Frederick Tyrrell, Esq.

The Hunterian Society holds its meetings every alternate Wednesday evening throughout the year, at No. 10, Saint Mary Axe, where communications on any department of medical science, addressed to the secretaries, will be thankfully received.

On the 10th of June was published, the First Number of a Work entitled, HERBARIUM EDINENSE, or Dried Specimens of Plants, growing wild chiefly in the vicinity of Edinburgh. By JAMES ROBINSON SCOTT, F. L. S. Member of the Wernerian Society, Senior President of the Royal Medical Society of Edinburgh, Lecturer on Botany, and Surgeon Royal Navy; and WILLIAM JAMESON, Surgeon. This Work will be published quarterly, and will, when completed, contain 350 Species. Fifty copies only of each Number will be published. Letters addressed (*post paid*) to Mr Scott, No. 18, St Patrick Square, Edinburgh, will be attended to, as well as personal applications, from Ladies and Gentlemen desirous of obtaining further information relative to the Work, or of becoming subscribers.

Dr PEARSON and Professor BRANDE's Lectures will recommence the first week of October next, *namely*, the Medical Lectures at eight and nine in the morning on Mondays, Wednesdays, and Fridays, in George Street, Hanover Square: and the Chemical Lectures at nine in the morning, on Tuesdays, Thursdays, and Saturdays, at the Royal Institution.

Books received for review:

Observations on the Symptoms and Specific Difference of Venereal Diseases, interspersed with Hints for the more Effectual Prosecution of the present Inquiry into the Uses and Abuses of Mercury in their Treatment. By Richard Carmichael, M. R. I. A &c. 8vo, London, 1818, pp 221.

An Essay on the Diagnosis between Erysipelas, Phlegmon, and Erythema; with an Appendix, touching the Probable Nature of Puerperal Fever. By George Hume Weatherhead, M. D. Edinburgh. 8vo, London, 1819, pp. 72.

Practical Illustrations of the Progress of Improvement for the last thirty years: or Histories of Cases of Acute Diseases, as Fevers, Dysentery Hepatitis and Plague, treated according to the doctrine of Excitation, by himself and other practitioners, chiefly in the East and West Indies, in the Levant, and at Sea. By Charles Maclean, M. D. &c. 8vo, London, 1818, pp. 236 and xli.

Results of an Investigation respecting Epidemic and Pestilential Diseases, including Researches in the Levant concerning the Plague. By Charles Maclean, M. D. 2 vols. 8vo, London. Vol. I. pp. 492, 1817. Vol. II. pp. 524, 1818.

No. LXI. will be published on the 1st *October* 1819.

THE
EDINBURGH
MEDICAL AND SURGICAL JOURNAL.
1. OCTOBER 1819.

PART I.

ORIGINAL COMMUNICATIONS.

I.

RESEARCHES ON THE PATHOLOGY OF THE BRAIN.

PART IV.

ON THE ORGANIC DISEASES OF THE BRAIN.

By JOHN ABERCROMBIE, M. D. Fellow of the Royal College of Surgeons of Edinburgh.

WHEN urgent symptoms seated in the brain do not yield in a short time to active treatment, and, on the other hand, do not shew any tendency to come speedily to a fatal termination, we begin to suspect some of those permanent changes in the organization of the brain, which we include under the general term, organic disease. The morbid condition, in such cases, may exist under various forms. There may be a tumour external to the brain, and in its formation quite distinct from it. There may be a tumour imbedded in the substance of the brain; and there may be induration of a portion of the brain itself. To these are to be added, thickening of the membranes, ossifications, hydatids, and several other morbid appearances, which will be more particularly referred to in the sequel. The symptoms also appear under various forms; and they vary considerably in their progress. In some of the cases they are at first very slight, and advance slowly and insidiously for a considerable time before they assume an alarming appearance. Other cases commence with more violent symptoms, which have generally either an inflammatory or an apoplectic character. In these, the first urgency is relieved by the evacuations and other remedies that are employed; but we soon find reason to believe that the disease is not thus to be removed. The patient perhaps is becoming pale and exhausted under the remedies, while the affection of the head is but little abated. New symptoms probably begin to appear, such as important affections of the senses, convulsive or paralytic symptoms; and thus we are led to perceive that the complaint which at first had the appearance of an acute attack, has passed into organic disease. Organic affections of the brain also frequently supervene upon injuries; and in these cases we observe the same variety in the symptoms. The injury may have been slight, and at the time perhaps excited no alarm; and it is only after a considerable interval that we find reason to suspect that from that period some diseased action has been going on in the brain, which at last comes to produce symptoms of a most unequivocal and alarming nature; or the injury may have been severe, the immediate urgency of the symptoms may have been subdued by the appropriate treatment, and organic disease may have been left, as the result of the morbid condition that had thus been excited. In all these cases the disease may go on for a considerable time, producing a variety of symptoms to be afterwards mentioned, and may at last be fatal suddenly by convulsion; more slowly by coma, of several days continuance; or still more gradually by the patient being, without either coma or convulsion, worn out by protracted suffering.

In tracing the symptoms of organic disease of the brain, it is necessary to distinguish betwixt those symptoms which are properly connected with the disease itself, and those which immediately precede the fatal termination; and it is necessary to keep in view the changes which take place in the morbid appearances, in connection with these fatal symptoms. It is chiefly when the patient dies of some other disease, without any change in the disease of the brain, that we find the morbid condition of that organ in what may be termed its original state. When the disease is itself fatal, it is generally by an attack of chronic inflammation; and, in connection with this, important changes take place both in the seat of the original disease and in other parts of the brain. In the cases which will be referred to, these varieties will be observed. There are some in which, while the disease in the brain was going on, the patients died of other diseases, or were suddenly killed by accidents; and there are some in which they died suddenly in convulsion, without any symptoms that previously indicated perceptible change in the disease. There are some also in which they died gradually exhausted by protracted suffering, without any important change in the symptoms in the brain. But there are others in which, a short time before death, important changes took place in the symptoms, resembling an attack of chronic inflammation, and the patients were cut off by the usual course of this affection, terminating by coma of several days continuance. In these cases, besides the original organic disease, we find various morbid appearances connected with the fatal attack, such as effusion, suppuration, or a softened state of a part of the brain; (see case 11th;) and, in some of them, there is reason to believe that the organic disease itself, or part of it, has passed into suppuration. This probably occurred in cases 10th and 28th. In some cases, again, an attack of this kind may occur, and may be carried off by the ordinary treatment, the original disease then going on, until another attack of an inflammatory character is fatal. This probably occurred in cases 6th and 13th.

Another remarkable variety occurs, in which the organic disease seems to produce no urgent symptoms until the symptoms of chronic inflammation appear. This probably occurred in case 1, in which the symptoms commenced only six weeks before death, though the remarkable tumour that was met with on dissection must have existed for a much longer period.

In all the forms of the disease remarkable remissions of the symptoms occur, so that the patient and his friends may be deceived into a belief that no fixed disorder exists in the brain. Sometimes the pain occurs periodically, and with remarkable regularity, generally attacking in the early part of the night and going off towards morning. In other cases it is excited by various occasional causes; such as external heat, stimulating liquors, bodily exertions, agitation of mind, or even the excitement of cheerful conversation.

SECT. I.—SYMPTOMS OF ORGANIC DISEASE IN THE BRAIN.

When we attempt to arrange the organic diseases of the brain, according to their symptoms, they seem naturally to be divided into eight classes.

I. The first class is distinguished by long continued and severe headach, without any other remarkable symptom. The pain varies very much in its seat and in its severity; and, in some cases, it occurs in paroxysms so as to resemble periodical headach; in other cases there is constant pain, but with paroxysms of aggravation. It is sometimes an acute lancinating pain, darting across the head from temple to temple; sometimes a more obtuse and fixed uneasiness, often referred to some particular part of the head, as the crown or the back of the head. In many cases it is accompanied by a violent throbbing, and this may be general, or it may be referred to a particular part, as the back of the head, or one temple. In the more violent paroxysms the pain is intense, obliging the patient to remain for a considerable time in one position, the slightest motion aggravating it to perfect torture; and sometimes there is transient delirium. In the earlier stages, and in some cases through a great part of the course of the disease, the symptoms suffer remarkable remissions, so as to deceive a superficial observer in regard to the real nature of the disease, and often to make it be considered as periodical headach, sick headach, or headach connected with dyspepsia. The latter supposition is countenanced by the stomach being frequently much disordered, and the more violent attacks being often accompanied by vomiting. The diagnosis is sometimes difficult, but, by attention, it may generally be made with tolerable precision. The long duration and violence of the pain will lead to a suspicion that the complaint is something more than common headach; and though the stomach is at times much disordered, it will be found that the complaints in the head do not keep pace with the state of the stomach, but that they are often most severe when no disorder that can account for them exists in the stomach. The patient generally cannot bear a warm room, the noise of company, or the exertion of cheerful conversation, without being distressed, and his headach increased. The same effect is produced by wine and all bodily exertions. He seeks quietness, coolness, and darkness. In these respects the disease differs from dyspeptic headach, which is usually relieved or dissipated by activity and cheerful company. Sometimes the paroxysms are accompanied by vomiting, and sometimes by violent throbbing in the head. If, along with these severe local symptoms, the face be pale and the pulse feeble, as is often the case, and the whole appearance indicating the reverse of plethora, organic disease may be suspected. In many cases, however, the diagnosis is extremely difficult; and it is only by the severity of the pain and its obstinate resistance of the most powerful remedies, that we come to suspect the nature of the disease. I have already alluded to a remarkable modification of this form of the disease, (exemplified in case 1st,) in which extensive organic disease seems to exist for a considerable time without producing any urgent symptoms until a short time before death. The symptoms which then appear seem to be rather connected with an attack of chronic inflammation than to be immediately dependent upon the organic disease.

The terminations of the cases of this first class will appear from the examples. They may be fatal by coma with or without convulsion; suddenly by convulsion; by gradual exhaustion without either convulsion or coma; or, while the affection is going on, the patient may die of another disease.

The description now given of this first form of the disease applies more or less to all the other forms; they differ from it chiefly in the addition of other symptoms, which make the nature of the complaint much more distinct, and it is in the character of these additional symptoms that they differ from one another

II. In the second form, after some continuance of fixed uneasiness in the head, such as has now been described, the organs of sense come to be affected, and frequently the intellect. Every sense may be affected; most commonly the sight; frequently the hearing, sometimes the taste, and occasionally the smell. The loss of vision generally takes place gradually; the sight being first obscure, so that the patient sees dimly as through a mist, or with the outlines of objects ill defined; and after some time this terminates in blindness. One eye generally is first affected in this manner, and some time after it has become blind, the other begins to see indistinctly, and gradually becomes blind also. When both eyes are affected at once, the one is often affected in a greater degree than the other; the one pupil perhaps being observed dilated and immoveable, while the other appears much more healthy. One remarkable case will be mentioned, in which the blindness took place rather suddenly, and in which, some time after it had taken place, sight was restored under the action of an emetic, continued distinct for an hour, and then was lost for ever. In some cases double vision occurs, and it may either be permanent, or may occur only at intervals. In many cases of this class the intellect is impaired, and sometimes the speech is lost. In their terminations they admit of the same varieties which have been mentioned in regard to the former class. The morbid appearances present no uniformity. In two of them there were tumours so situated as directly to compress the optic nerves; in another a large tumour pressed upon the corpora quadrigemina; in a third, the disease was seated at the lower part of the anterior lobe; and in case 12th, in which the right eye only was affected, it was in the substance of the left hemisphere, near the posterior part. In one by Drelincurtius, the disease was an enlargement of the pineal gland, and in another, in which there were both blindness and deafness, a large tumour was seated between the brain and cerebellum.

III. The third class corresponds with the second, in the pain and affections of the senses, with the addition of paroxysms of convulsion. These may occur with some degree of regularity like epilepsy; but in other cases they only occur at particular times, when, from causes which elude our observation, the disease seems to be aggravated and combined with an inflammatory tendency, (case 13th.) In case 19th the paroxysms had a resemblance to tetanus. In case 21st they rather resembled slight apoplectic attacks. In this case there was loss of sight, hearing, smell, and speech, and at last of the power of deglutition.

The cases of this class present the same varieties of morbid appearance as the former; no diversity appears in them that can account for the difference of symptoms betwixt the second and third classes; and when we compare both these with the first class, nothing appears to explain the remarkable difference in the symptoms, except in those cases in which the disease was so seated as immediately to compress the optic nerves. This seems invariably to produce blindness; but in the other cases it will appear that tumours similarly situated, and nearly of similar size, were, in some cases, accompanied by blindness and convulsion; in others, by blindness without convulsion; and in others, by pain without either of these affections. On what these remarkable differences depend, remains to be investigated by extensive and accurate observation. Case 18th is remarkable from the circumstance, that the convulsions ceased when the lungs became diseased, and did not return; the affection of the lungs being fatal.

IV. The fourth class is distinguished by convulsion, without any affections of the senses. In many of the cases there is little complaint of pain; and, in general, there is not that fixed and constant pain which occurs in the other classes. The convulsion in some cases occurs under the usual form of epilepsy; in others, in more irregular attacks, occurring repeatedly for a short time, and then ceasing for a considerable interval. Sometimes violent paroxysms of headach precede or accompany the attack of convulsion. After some time the memory is generally impaired. These cases may be fatal suddenly in one of the fits, or by coma of some days continuance. The morbid appearances, as will be seen from the cases, are various. A tubercle in the cerebellum,—an hydatid in the right hemisphere of the brain,—induration of the pons Varolii, or of the substance of the brain,—tumours and bony spiculæ in various parts of the brain, are the most remarkable. An important variety of the disease occurs in which the convulsion is confined to one side of the body. Two cases of this kind are referred to; in one of which there was a tumour on the surface of the opposite hemisphere; and in the other, a portion of the opposite hemisphere was indurated. The former case was also remarkable from the long continuance of the disease, and from the weakness and diminution of size of the lower extremity of the affected side.

V. & VI. The fifth and sixth classes lead our attention to a new set of symptoms,—the paralytic. These may occur in the form of hemiplegia, paraplegia, or paralysis of all the parts below the neck. In some cases one limb only is affected. The disease is distinguished from the ordinary apoplectic paralysis by the affection coming on gradually,—one limb, perhaps, or part of a limb, being first weak, and the weakness extending by degrees over that limb and the corresponding one, and increasing very gradually till it amount to paralysis. In one of the cases the paralysis was preceded by violent pain in the limb. In other respects the cases of this class may resemble several of the other classes. There may be fixed uneasiness in the head, or there may be headach occurring in paroxysms. The speech is generally affected, and in many cases the memory. In one case there was blindness of one eye; in another, of both. In one there occurred convulsion, but not till an advanced period; in another, epilepsy for more than a year.

The dissections exhibit tumours or indurations variously situated; in the cases of hemiplegia, on the opposite side of the brain; in those of paraplegia, in the cerebellum or tuber annulare. In a remarkable case (33) of universal paralysis, with the mind entire to the last, the tumour was in the posterior part of the left hemisphere; in another, in which the disease was on the tuber annulare, there was first hemiplegia, which was recovered from, then paraplegia, and at last universal paralysis. Extensive effusion occurred in several of them. This is probably to be considered as an effect of the disease.

VII. The seventh class presents to us a subject of much interest; a train of symptoms which are very apt to be referred to the stomach, but which really depend upon disease in the brain. In many of the cases of organic disease the stomach is affected, but those to which I now allude are remarkable from the affections of the stomach being the most prominent symptom, and from there often being, through a considerable part of their progress, but little complaint of the head, or no complaint there so fixed and urgent as to direct our attention to the brain as the seat of the disease. There is generally, however, in these cases, some complaint of pain or weight in the head, sometimes occurring in paroxysms like periodical headach, or in paroxysms accompanied by vomiting, like what is called " sick headach." The pain is increased by exertions, external heat, passions of the mind, and stimulating liquors. There is generally variable appetite, bad sleep, oppression of stomach, acidity, and occasional vomiting. The vomiting sometimes occurs in the morning on first awaking, and sometimes it occurs at uncertain periods, and without any warning, occurring so rapidly that a patient will sometimes discharge the contents of his stomach upon a book which he is reading. There are various uneasy sensations, which are sometimes referred to one organ, sometimes to another; and the disease has been often ascribed to derangement of the functions of the liver, or disordered action of some of the chylopoietic viscera. In this manner the case may go on for a considerable time before its real nature is fully ascertained. After some time, however, symptoms begin to appear which point out its dangerous character, such as fits of loss of recollection, affections of sight, convulsive paroxysms, and in one of these death may occur, in some cases unexpectedly, in others preceded by a train of severe suffering. In cases of this class we must beware of being misled in regard to the nature of the complaint, by observing that the symptoms in the stomach are alleviated by attention to regimen, or by treatment directed to the stomach itself. If digestion be impeded, from whatever cause, the uneasy symptoms in that organ may be alleviated by great attention to articles of diet, but no inference can be drawn from this source in regard to the cause of the derangement. In the earlier periods of these cases the diagnosis is often difficult. There is generally more fixed and permanent uneasiness in the head than we should expect to find in a dyspeptic case, and the uneasiness is increased by causes which would probably be beneficial to a dyspeptic headach, such as activity or cheerful company. The uneasiness in the head, also, is not regulated by the state of the stomach; but may be most severe, when from circumstances we have reason to believe the stomach to be in good order.

The morbid appearances connected with this class are various

The most remarkable in the annexed cases were in the cerebellum.

VIII. The eighth class is distinguished chiefly by those slight and transient apoplectic affections which I have alluded to when treating of apoplexy. I have formerly given several remarkable examples of these, connected with organic disease in the brain, and shall in this part of the subject add several more, illustrative of the different forms of the disease. In some cases it consists chiefly of an habitual giddiness, which makes the patient afraid to walk alone; in others there are sudden attacks of loss of all muscular power, which are soon recovered from and without loss of recollection. Sometimes there are attacks of perfect coma, which may occur at regular or at irregular intervals; the patient in some cases having such warning of their approach, that he lies down and immediately becomes comatose. There is usually more or less unsteadiness of the limbs, and generally, but not always, headach. In some cases there are affections of sight, and these may be either permanent or may occur in paroxysms. Sometimes urgent symptoms, such as giddiness and loss of recollection, are excited by bodily exertions, and go off on desisting from them. I refer to a former dissertation on apoplexy for the doctrine which I have proposed in regard to the pathology of these affections.

SECT. II.—OF THE NATURE OF THE ORGANIC DISEASES OF THE BRAIN.

The various forms of disease which have now been described are found to be connected with great variety of morbid appearances in the brain; but much observation is still wanting in regard to the minute characters of these appearances, and the connection betwixt particular forms of them, and particular varieties of the symptoms. The following are the most prominent appearances.

I.—Tumours formed by circumscribed depositions of coagulable lymph on the surface of the brain. These, I think, are generally formed under the dura mater, and they may be found in various steps of their progress, from recent depositions of a gelatinous appearance, to more firm and organized masses. In case 16th, of the first part of these essays,[*] I have described a remarkable tumour, formed by deposition of this kind, between the laminæ of the dura mater; and in different parts of that tumour, the different steps in its formation were distinctly traced, some parts of it being in the state of clear recent gelatinous matter, others firm, dense, and opaque. In a case quoted from Lancisius, in the second part, * a tumour, which was probably of this kind, occurred under the frontal bone; and there is another example of it in a case of epilepsy, from M. Poupart, referred to under that head in this paper. It is described as a thick, white substance, harder than jelly, under the dura mater. This affection probably proceeds from a state of chronic inflammation of a part of the dura mater, and is commonly accompanied by thickening of that membrane, at the part which covers the deposition.

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[*] On Chronic Inflammation of the Brain and its Membranes, Edinburgh Medical and Surgical Journal, Vol. XIV. p. 291.

II.—A very dense tumour growing from the dura mater, externally and internally of a uniform white appearance, like coagulated albumen. This does not appear in irregular masses like the former, but in distinct rounded tumours of various sizes. I have described one that grew from the falx, which was nearly five inches in circumference. They do not appear to be covered by any sac, but to be a production from the surface of the membrane. The large one which I have now referred to had, when analyzed, the properties of coagulated albumen. It was uniformly white and firm, and did not appear to be organized. The tumour growing from the tentorium in case 9th had nearly the same external appearance, but internally it was of a reddish colour, and appeared to be organized. The symptoms connected with tumours of this kind will be seen from the cases to be very different in different cases. Sometimes they arise from the outer surface of the dura mater; in this case they have frequently been known to produce absorption of the bone, and have been found rising externally under the integuments, so as to be mistaken for wens. I have given an example of this kind of tumour in case 25th. Many others are mentioned by the French writers. † In many of them the disease seems to have been originally induced by injuries; and in others an injury seemed to have accelerated the process of caries by which the tumour was making its way through the bone. When these tumours have been rashly meddled with by incision, death has generally been the consequence.

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* On Chronic Inflammation of the Brain and its Membranes, Edinburgh Medical and Surgical Journal, Vol. XIV. p. 577.

† See Mr John Bell's Principles of Surgery, Vol. II. p. 396, et seq.

Small tumours which resemble those of this class are sometimes met with attached to the choroid plexus. I have seen one in each lateral ventricle the size of small hazel nuts in a man who died suddenly after having had repeated epileptic attacks at long intervals, and having been for some time affected with symptoms threatening apoplexy. Dr Baillie has delineated a similar appearance in one of his engravings.

III. A tumour on the surface of the brain, and distinct from the cerebral substance, inclosed in a sac, and with a slight attachment to the surrounding parts. Tumours of this class lie under the dura mater, and the sacs of them seem to be derived from the arachnoid membrane or the pia mater. The structure of them is variously described; some are compared to a scrofulous gland; others are said to be of a fatty appearance; others are reddish and more distinctly organized. They may be found in a state of suppuration, as in case 10th. They probably originate in enlargements of the small glandular-looking bodies which are found on the surface of the brain. They vary in size, and are sometimes met with as large as an egg.

IV. Particular parts of the brain in a state of enlargement, as the pineal gland in case 6th, the pituitary gland in case 10th.

V. Morbid changes in the substance of the brain or the cerebellum. I have formerly proposed an opinion that the diseases which come under this head are the result of chronic inflammation affecting a small part of the brain; and I have endeavoured to trace its progress from the state of simple inflammation to that of induration and final suppuration, in which it often terminates. In its early stage I believe it is a disease that may be cured, and that it may continue a considerable time in this state,—that after a certain time it passes into induration, and that probably there is in some cases formed around the indurated portion, a sac of coagulable lymph. In this state it may continue for a long time producing urgent symptoms, and may at last be fatal by passing into suppuration, or without having suppurated. According to the stage of its progress, therefore, in which death takes place, we may find a portion of the brain in a state of recent inflammation, or indurated, and of a red or yellowish colour; or we may find a part of it hard and a part suppurated; or we may find the diseased part entirely suppurated. The simple induration we find chiefly when the patient dies of some other disease. When the disease itself is fatal, it is generally by a fresh inflammatory action, and thus the indurated part often passes into suppuration. We find, accordingly, in such cases, other marks of the inflammatory state, as deposition of coagulable lymph and serous effusion. The symptom attending this form of the disease, both in the brain and cerebellum, will be seen from the cases. The striking distinction in the appearances is betwixt simple induration of the cerebral matter and tumours imbedded in the substance of the brain. The term tumour seems to have been by many applied to both, so that there is a great want of precision on this part of the subject. That which is properly called a tumour in the substance of the brain, perhaps should have a sac by which it is separated from the cerebral matter; and it remains to be investigated whether such a tumour is a portion of the brain in a diseased state, and around which a sac of coagulable lymph has been formed by an inflammatory process, or whether a distinct tumour may be formed in the substance of the brain of the same kind as those which are formed on the surface.

VI. Ossifications. Under this head are to be included both osseous projections from the inner surface of the cranium, and internal ossifications, which are commonly formed in the dura mater, most frequently in the falx. An example of the former kind occurred in case 16th, connected with headach, convulsions, and blindness; and another, in a case quoted from Mr Gooch under the 8th class, in which there were frequent attacks of an apoplectic appearance combined with a convulsive affection resembling emprosthotonos. The internal ossifications usually appear in the form of thin laminæ of bone in the substance of the dura mater, most commonly in the falx, where they are sometimes found of considerable thickness. Of such ossifications there are many examples on record; some of them in cases in which the patients had enjoyed good health until cut off by some incidental disorder; others in which there had been urgent symptoms distinctly referable to the brain; and others in which there had been a long train of obscure complaints, and no other morbid appearance but these ossifications were met with on dissection. The latter cases were chiefly affections of the stomach, with some headach; and some examples of it will be found under the 7th class of cases. It is probable that ossifications produce urgent symptoms in the brain, when they are in the form of sharp spiculæ, or have acute angles, which are so directed as to irritate the brain or the membranes. An example of this occurs in case 8th, in which a part of the dura mater, which was in contact with an angle of the ossification, was evidently inflamed. Another, connected with epilepsy, is quoted from La Motte under the 4th class of cases, in which there were bony spiculæ directed against the pia mater and a third, from Van Swieten, under the same class, in which there was in the substance of the cerebellum an irregular piece of bone an inch long, and half an inch broad.

VII. *Hydatids.* This name has been applied to several appearances in the brain, some of which do not appear to be really hydatids. Of this kind are the vesicles which are frequently met with in the choroid plexus; they seem to be merely the loose cellular texture of that organ elevated into vesicles by watery effusion. In a case by Dr Baillie they could be injected from the veins. Real hydatids, however, seem to occur in the brain, as in a case quoted from Zeder, under the 8th class, in which there were numerous hydatids, one, particularly, the size of an egg, which contained three smaller hydatids within it. There are many examples of cysts in various parts of the brain, containing a watery fluid, but it is doubtful whether they are to be considered as hydatids. A cyst of this kind occurs in case 21st, and another in case 23d, in which the fluid was yellowish, and partly gelatinous. A woman mentioned by Bonetus, after a blow on the back of the head, had headach for a year with constant vertigo, and then died apoplectic. In the substance of the left hemisphere there was a cyst the size of an egg, which contained a clear fluid. (Boneti Sepulchretum, Vol. III. p. 319.) In a priest who had become stupid and died apoplectic, there were found certain round bladders full of a pituitous fluid above the corpus callosum. (Ib. Vol. I. p. 258.) In a girl, mentioned by the same writer, who died on the 12th day of a febrile disease which ended in coma, there was found, on separating the two hemispheres, a cyst which burst on a slight touch, and discharged limpid fluid to eight or nine ounces. (Ib. p. 172.) Bonetus mentions other cysts of a much larger size, one especially, attached to the nates and the infundibulum, which burst on taking it out, and discharged fluid to the amount of lb. ij. The patient had complained of fixed pain in the vertex for four months. I was long inclined to doubt the accuracy of these accounts, and to suppose that, in such cases, the fluid had been really contained in the ventricles, until I received, through my friend Mr Frampton, the remarkable case by Mr Headington, (No. 29,) in which an hydatid or cyst was found in the left ventricle that contained ℥xvj of fluid.

I think it probable that some of these watery tumours are formed by effusion betwixt the laminæ of the membranes, or in cysts formed by doublings of the pia mater, connected with an inflammatory process. The possibility of such an occurrence appears from the case of a boy mentioned by Mr Paisley, who had fixed pain at a particular spot on the left side of the head, followed by coma, and died in 12 days. There was much effusion and other morbid appearances in the brain; and on the surface, exactly corresponding to the seat of the pain, there was a tumour the size of a large hazel nut, formed by a separation of the laminæ of the dura mater, and the deposition between them of a bloody serous fluid. (Edinburgh Medical Essays, Vol. III. p. 265.) In case 12th I have described a singular cyst found in the substance of the left hemisphere, full of a pure albuminous fluid.

Several tumours are mentioned which do not belong to any of these classes; the stony tumour, for example, in case 5th, and in the case quoted from Schenkius; the bloody tumour of Rochoux in case 28th, and some others. Of these little is known; the latter, perhaps, was of the nature of fungus hæmatodes. An affection of this kind has been observed in the brain, and fungous protrusions from the substance of the brain. These are familiar to us after the operation of trepan, but may also occur without any injury. A man mentioned by Dr Donald Monro, had a pulsating tumour above the left eye-brow, pressure on which produced headach and giddiness. He could give no account of the origin of it, but that he first observed it six months before, and it was then two fingers-breadth in diameter. It increased in size, and after seven months he died lethargic. On dissection the tumour was found to arise from the anterior part of the left hemisphere of the brain, and to have made its way through the frontal bone. It was the size of a China orange; its structure resembled the medullary substance of the brain, and it appeared to be covered by the dura mater. (Trans. of the Coll. of Physicians, I. 353.)

CASES AND ILLUSTRATIONS.

SECT. I.—FIRST CLASS.—Long continued headach, terminating at last by coma, or by gradual exhaustion, or the patient dying of some other disease.

1. A boy, aged 7, had been for more than a year affected with scrofulous sores, and during that time had been in a declining state, without any urgent symptom, till six weeks before his death, when he began to complain of pain of the abdomen, and a fixed and severe pain of the forehead. His pulse was natural, but his appetite was bad, his tongue foul, and his sleep disturbed. The pain of the belly was removed by purgatives, but that of the forehead continued very severe, so as sometimes to occasion screaming. The usual remedies

having been employed, the pain was much alleviated, and for a fortnight he made little complaint. The headach then returned with much severity, and, without any other change in the symptoms, he gradually sunk into coma, and died in three days. *Dissection.*—Much fluid in the ventricles, and the substance of the brain in several places very soft and much broken down. A large firm white tumour adhered by its base to the middle of the falx at its lower part on the right side. It was nearly five inches in circumference at the broadest part, and about an inch and a half in thickness. Internally it was uniform, white, and firm, like coagulated albumen. It was imbedded in the substance of the right hemisphere where it had formed a depression for itself, but without any adhesion to the substance of brain. Its attachment to the falx was at the very lower part, and part of the tumour descended lower than the edge of the falx. There were two smaller tumours the size of hazel nuts, but of the same appearance, attached to the falx, one at its posterior and the other at its anterior extremity, both on the right side; and a fourth, also small, imbedded in the anterior lobe of the left hemisphere, and attached by a slender filament to the pia mater.

2. A boy, aged 14, affected with scrofulous disease of the knee, was seized with severe headach, which continued without any other symptom for two months. He was then seized with convulsion, followed by coma, and died on the 8th day. *Dissection.*—Serous effusion both in the ventricles and on the surface of the brain. Behind the posterior part of the medulla oblongata, there was a tumour the size of a walnut, of a rose colour and a fatty consistence, intersected with red lines. It was inclosed in a thin sac, and adhered very slightly to the neighbouring parts. There was another of a smaller size in the substance of the left lobe of the cerebellum. The mesenteric glands were enlarged, and the small intestines in several places ulcerated. (Edinburgh Journal, II. 405.)

3. A man, aged 35. *Symptoms.*—Consumptive complaints and scrofulous sores; violent headach, which was sometimes so severe as to oblige him to remain for a considerable time in one posture, the slightest motion aggravating it to perfect torture. In this state he continued seven months, and then died of complete marasmus, without any change in the head symptoms. *Dissection.*—℥iij. of fluid in the ventricles of the brain; a firm tumour the size of a pigeon's egg at the upper and middle part of the right hemisphere. It was inclosed in a reddish sac, and was internally of a yellowish colour. A larger tumour of the same appearance in the left lobe of the cerebellum. Extensive disease of the thorax and abdomen. (Ib. 406.)

4. A woman, aged 50. *Symptoms.*—Violent and constant pain under the sagittal suture for six months; then a lethargic state from which she recovered, the pain continuing as before for three weeks, when the lethargy returned, and was fatal. *Dissection.*—A tumour three inches broad, adhering to the membranes by the side of the third sinus; (the left lateral sinus?) effusion in the brain both externally and internally. (Willis, An. Brut. Pars II. Cap. 2.)

5. A noble lady. *Symptoms.*—Long continued headach, varying in degree, sometimes occurring in paroxysms which obliged her to scream and grasp her head with both her hands, the pain being such as if a sharp instrument were driven through her head. She died of dropsy. *Dissection.*—Under the dura mater, near the torcular, there was found a hard, rough, irregular mass of a stony consistence, (lapideam,) with many sharp processes and angles. It adhered firmly to the dura mater. (Borellus, App. Obs. xv.) A similar case by Schenkius, I. 113. A stony tumour like a mulberry in the brain of a man who had suffered from long continued and intense headach, which left him no interval of ease day or night.

6. An officer, aged 33. *Symptoms.*—Slight pain or confusion of head, with impaired appetite. After ten weeks nausea and pain in the balls of his eyes, especially when he turned them to either side. He was then wounded in the head; lost much blood; bone exfoliated, then healed. He was better for more than a year; was then attacked with headach, watchfulness, flushing, and ophthalmia. Recovered after three months, and was able for his duty, but never free from headach. It gradually increased; was sometimes referred to a spot on the occiput, and sometimes through the whole head. Was aggravated by motion, which produced a painful jarring in his head, and very much increased by going to stool. Pulse from 80 to 90. Pain at last became excruciating, with numbness of the left hand. Became suddenly delirious, then comatose, and died, having been confined to bed but three days. Whole duration of the complaint three years. *Dissection.*—℥iij. of fluid in the ventricles. In the seat of the pineal gland, a little to the right side, there was a firm tumour, the size of a nutmeg; internally it was like cheese, but organized. Cerebellum very soft and broken down. (Trans. of Soc. II. 198.)

7. A young man, of 19 or 20, had violent pain in the forehead. It occurred in paroxysms, which generally continued four days, and were followed by intervals of partial relief of about the same duration. After two years he died suddenly in the night, in convulsion. *Dissection.*—In the substance of the cerebellum there was a tumour the size of a pigeon's egg, hard in the centre, and externally soft. All the other parts healthy. (Planque, Biblioth. III. 348.)

8. A man, aged 35, had been for a year affected with so violent a pain in his head that he sometimes lost his reason. *Dissection.*—Under the junction of the lambdoidal and sagittal sutures, there was at the broadest part of the falx a small triangular piece of bone with very sharp angles. Where these angles came in contact with the dura mater, it was livid, and discharged a little pus. There was effusion in the ventricles. (Saviard, Chirurg. Obs. vi.)

SECT. II.—SECOND CLASS.—Pain, Affections of the Senses, Speech, or Intellect.

9. R. N. a shoemaker, aged 47. *Symptoms.*—Headach, and a feeling of weight in the head, which began in May 1816, increased by stooping, and even by the posture in which he sat at his work; increased gradually, notwithstanding copious evacuations. In August, had giddiness and dimness of sight; pulse natural. In September, sight more affected, could only see objects in a very strong light. In October, several attacks of delirium in the night time; pain still constant and severe; sight continued to diminish, and by the end of December was lost. About the middle of January, a degree of stupor and forgetfulness, pupil insensible. Died comatose on 31st. *Dissection.*—About ℥iv. of fluid in the ventricles. A tumour, the size of a large egg, was attached to the tentorium, in such a manner that part of it lay above it, and part below it, the anterior part of the tentorium entering into its substance, to a considerable depth on its posterior part. On its upper part, the falx near its termination entered into it in the same manner. Internally, the tumour was firm, organized, and in colour resembled the structure of the kidney.

10. An officer who had seen much service. *Symptoms.*—Severe headach, which subsided after some time, leaving a feeling of tightness across the forehead. After six or eight months, sudden diminution of sight of the right eye, and soon after blindness of it; then blindness of the left eye. Under the operation of an emetic the sight of the right eye was recovered for an hour; after this continued perfectly blind, and pupils insensible, but had no other complaint except disordered stomach, and a frequent disposition to vomit. Pulse natural. Died comatose, two years after the commencement of the disease. *Dissection.*—℥iv. of fluid in the ventricles. A tumour the size of a hen's egg, containing a thick purulent fluid, was situated under the anterior part of the brain, and interposed between the optic nerves, which were much separated from each other by it. Below, it was attached to the pituitary gland, which was very soft, and enlarged to five or six times its natural size; posteriorly the tumour extended into the third ventricle. (Med. Trans. V. 223.)

11. A man, aged 30. *Symptoms.*—Excruciating headach, which commenced after hard work in a hay field. The pain was chiefly referred to the forehead, from which it extended over the left ear, but sometimes affected the right side of the head also, and occasionally the neck; most severe in the night; violent throbbing in the head. After six weeks his sight was much impaired, and soon after lost; face a little twisted to the left side; pulse little affected. Two months from the beginning of the complaint, he had an apoplectic attack, and died in two days. *Dissection.*—A tumour the size of a large walnut projected from the lower part of the anterior lobe of the left hemisphere of the brain. Internally it resembled an absorbent gland. The greater part

of the medullary substance of the left hemisphere was reduced to a soft pulpy state, and was of a light brown colour. (Med. Trans. V. 241.)

12. A. M. a shoemaker, aged 50, for sometime affected with cough and bloody expectoration. In June 1818 was affected with headach and some confusion of thought, which appeared chiefly in a tendency to misapply words. The pain increased, attacked him in violent paroxysms, and was referred to the forehead. The sight of the right eye was impaired, and soon after lost; his speech became indistinct, and after sometime inarticulate. Six weeks after the beginning of the complaint, all the symptoms were increased. Violent paroxysms of pain were excited by the least motion, even by change of posture in bed He seldom attempted to speak, but often pressed his hand on his forehead, and he seemed to have uneasiness in his right arm. He had some squinting; general weakness and paleness; intellect impaired. Died comatose in August. Pulse had been usually natural, sometimes slow. *Dissection.*—In the substance of the left hemisphere of the brain, towards the posterior part, there was a soft and vascular sac, containing about ℥ij. of a thick colourless albuminous fluid, coagulable by heat, and exactly resembling the albumen of an egg. The cerebral substance around the sac was softened; the brain in other respects was healthy. The ventricles contained a small quantity of serous fluid, and had no communication with the sac.

Many other examples of this class are on record, which it is not necessary to describe particularly, the symptoms being in all very similar, viz. fixed pain in the head, gradual loss of sight, generally first in the one eye and then in the other after some interval, the intellect being frequently impaired in the advanced stages, and the disease fatal, either suddenly by convulsion, by coma of a few days continuance, or by gradual exhaustion. In a case by Platerus, fatal by gradual wasting, there was a tumour larger than an egg, compressing the origin of the optic nerves. In one by Drelincurtius, there was a steatomatous tumour, the size of a fist, between the brain and the cerebellum. In this case there were both blindness and deafness, and it was fatal suddenly by an apoplectic attack. In another, by the same writer, the pineal gland was enlarged to the size of an egg, and was of an earthy or stony structure.

SECT. III.—THIRD CLASS.—Pain, Affections of the Senses, and Convulsion.

13. A girl, aged 11, long liable to headach, and affected with weakness of sight, and a peculiar tenderness of the integuments of the head, in autumn 1814 received an injury of the forehead from a fall, and from that time suffered much from headach, with frequent epistaxis. About the end of December, headach increased in violence, with fever, intolerance of light and sound, squinting, and convulsive

paroxysms, which for some time occurred every half hour. In March 1815, a great amendment took place, and for nearly a year she continued better in regard to the head symptoms, but affected with scrofulous sores on the neck, and on the leg. In May 1816, headach increased, with impatience of light and sound, squinting, gradual failure of sight, and at last blindness in July. She died in October, her intellectual faculties having continued unimpaired. Great intolerance of sound continued to the last, and remarkable acuteness of hearing. *Dissection.*—A tumour the size of a walnut rested on the sella turcica, and compressed the junction of the optic nerves. It was composed of a medullary substance of a yellowish colour, and was covered by a thin and delicate membrane. Considerable effusion in the ventricles.

14. A lady, aged 40, of a scrofulous habit. *Symptoms.*—Gradual failure of memory, of sight, and hearing; inarticulate speech; epileptic paroxysms, at first once in the fortnight, afterwards more frequent; her gait feeble and tottering; died in six months. A year before her death, she had been much stunned by a fall down a stair. *Dissection.*—A tumour the size of a small orange lay on the pars petrosa of the left temporal bone, inclining to the opposite side, and producing a great depression in the substance of the brain. The 7th pair of nerves, and the branches of the 5th pair, were compressed and stretched by the tumour. Internally it consisted of a soft uniform substance, resembling the cineritious matter of the brain.*

15. A young man, aged 15. *Symptoms.*—Deep-seated pain in the head. After six months inarticulate speech; then three attacks of convulsion, each of ten or fifteen minutes duration; the last left paralysis of the right side, which disappeared next day. Died comatose in another month, having had repeated convulsive attacks, intense headach, impatience of light, afterwards dilatation of the pupils, deafness, paralysis of the eyelids, very difficult articulation, and some delirium. Pulse slow. *Dissection.*—Membranes of the brain very vascular; ℥iv. of fluid in the ventricles. On the left side of the pons Varolii there was a hard tumour the size of a bean; cerebral substance round it was soft, approaching to suppuration. (Coindet sur l'Hydrenceph. p. 98.)

16. A man, (age not mentioned.) *Symptoms.*—Severe headach, followed by amaurosis and epileptic paroxysms, which occurred almost daily; died in an apoplectic attack. *Dissection.*—On the inner surface of the left parietal bone there was an osseous spongy tumour, three inches broad, and more than an inch in thickness. (Wepfer, p. 508.)

17. A man, aged 35. *Symptoms.*—Severe pain, and sense of weight

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* For Cases 13th and 14th I am indebted to Dr Hay, under whose care they were.

in the head for two years; copious epistaxis; lost the sense of smell; then frequent epileptic paroxysms, which continued for two years; died suddenly. *Dissection.*—On the anterior part of the right hemisphere, the substance of the brain was hard and callous, and adhered intimately to the dura mater. On the left side there was some extravasated blood. (Morgagni, Ep. ix. § 25.)

18. A man, aged 24. *Symptoms.*—Severe headach, with fever. Fever ceased, headach remained, with watchfulness and imbecility of the head, (vertigo?) Blindness of the left eye, and after a month of the right also. Convulsive paroxysms, which continued to recur for half a year; they then ceased, and he died of cough, with purulent expectoration, hectic fever, and wasting. *Dissection.*—Extensive disease of the lungs; much effusion in the brain. In the substance of the left hemisphere, there was a tumour larger than an egg, covered by a firm sac; internally it was white, firm, and uniform, resembling coagulated albumen, but harder; it weighed 14 drachms. (Fel. Platerus, L. I. 108.)

19. A man, aged 26. *Symptoms.*—Severe headach, most distressing when lying on left side, and giddiness; occurring in paroxysms. After several months, pain increased, with defective vision; dilated pupil; pain increased by motion; paroxysms of giddiness, with loss of speech and blindness during the paroxysm, and stiffness of the limbs: then double vision; violent pain in the neck, with convulsive paroxysms, affecting the muscles of the neck, and drawing the head violently backwards. Two months after this, he had numbness and spasmodic motions of the superior extremities. Died suddenly in a fit resembling epilepsy seven or eight months after the commencement of the disease. *Dissection.*—A hard tumour, two inches long, and an inch and a half broad, was firmly attached to the tentorium, and imbedded in the posterior lobe of the left hemisphere of the brain. It contained ℥i. of greenish pus. Substance of the brain near it was very soft, and broken down. Where the tumour pressed against the occipital bone, the dura mater was obliterated, and the bone rough. ℥ij. of fluid in the ventricles. (Clarke, Ed. Journ. VI. p. 275.)

20. A woman, aged 23. *Symptoms.*—After suppressed menstruation, violent headach, imperfect vision, dilated pupils, and after some time blindness. Repeated convulsive attacks, after one of which, she lost her speech for two days. Died comatose about four months after the commencement of the complaint. Intellect entire, till a few days before death. *Dissection.*—On the surface of the right hemisphere there were three hardened spots, each an inch in diameter. They constituted a part of as many tubercles which extended into the medullary substance of the brain. A similar tubercle was imbedded in the substance of the hemisphere, and one smaller on the surface of the left hemisphere; slight effusion in the ventricles. (Powel, Med. Trans. V. 219.)

21. A woman, aged 19. *Symptoms.*—Headach, vertigo, suffusion of the eyes; paroxysms in which she fell down insensible without con-

vulsion. They attacked her once in three weeks, and at each time there were two paroxysms at the distance of twelve hours. After eight or nine months these attacks increased in severity. She lost her hearing, and then her sight, first of the right eye, and after fourteen days of the left also; then lost her smell. Speech and deglutition were much impaired. Soon after this died apoplectic. *Dissection.*—Imbedded in the substance of the right hemisphere, there was a hydatid three inches long and two broad, and very vascular; brain in other respects healthy. (Med. Ch. Trans. II. 260.)

SECT. IV.—FOURTH CLASS.—Convulsions without affections of the Senses. Intellect sometimes impaired.

22. A woman, aged 43. *Symptoms.*—Headach; spasmodic affections of the limbs; after seven months was confined to bed; violent paroxysms of headach; loss of memory. Convulsive attacks more frequent; at last several times a day. Died suddenly in one of them, six or seven weeks after she was confined to bed. *Dissection.*—℥iv. of fluid in the ventricles. A tubercle the size of a nut in the left lobe of the cerebellum; surrounding substance much softened. (Rochoux sur l'Apop. p. 151.)

23. A man, aged 60. Epileptic for six years, with loss of memory. Died suddenly. *Dissection.*—A hydatid the size of a pigeon's egg in the substance of the posterior part of the right hemisphere. It contained a yellowish fluid, which was partly gelatinous. Substance of the brain under it was hardened. (Lancisius de Sub. Mort. Cap. xi.)

24. A boy, aged 16 months, formerly healthy, was seized with an attack of convulsion. It affected chiefly his right side, which was very strongly convulsed. He had a second attack on the evening of the same day, after which he had fever, blindness, and loss of the power of deglutition. In this state he continued, supported by glysters, for ten days, when he had another fit; and after this he gradually recovered the power of vision and deglutition. From this time there was a degree of paralysis of the right side, but in other respects, he enjoyed good health for four years. He then became affected with epileptic paroxysms, which at first occurred once in two months, but gradually increased in frequency. After a longer interval than usual, a few weeks before his death, the fits returned after a fright, recurred with great frequency, and were fatal. He was then twelve years of age; his right side had continued weak; and at the time of his death, the right lower extremity was three inches shorter than the other. His intellectual faculties had been weak, so that he never could be taught to read. *Dissection.*—On the surface of the brain, under the left parietal bone, there was a tumour the size of an egg. It was situated between the pia mater and the arachnoid membrane. Internally, it was of a white, and somewhat gelatinous appearance,

but very firm, and when cut into, some serous fluid was discharged from it; no effusion.*

25. A woman, aged 50. Epileptic for 30 years. Was seized with a cough, which excited acute pain under the upper part of the occipital bone. In one of the epileptic fits she received a violent blow on this spot. From this time she was never free from suffering; and after six weeks, a tumour appeared on the spot, which pulsated. It could be pressed back into the cavity of the cranium, and this produced coma. After several months, she had palsy of the right arm, and both lower extremities, and soon after died. *Dissection.*—The tumour was generated in the substance of the dura mater. It projected inwards into a hollow on the left hemisphere, and outwards through an opening in the bone, which was two inches in its long diameter, by one and a half. The dura mater near it was remarkably vascular. (Marigues, Mem. de Chir. p. 26.)

It is unnecessary to detail particularly the cases of this class. They present the usual symptoms of the epileptic paroxysm, occurring at various intervals, and in various degrees of severity, sometimes accompanied by violent attacks of headach, and sometimes with little uneasiness in the head. They are sometimes fatal suddenly in one of the fits, and sometimes by coma. A variety of morbid appearances have been observed in such cases. Portal found a remarkable induration of the pons Varolii. In a case by Sandifort, there were three tubercles on the dura mater, near the sagittal suture, on the right side, and in the anterior part of the right hemisphere, a sebaceous tumour, the size of a walnut. In a case by Lieutaud, there were seven sarcomatous tumours near the longitudinal sinus; and in another a glandular tumour, the size of a bean, in the substance of the right corpus striatum. In a young man, mentioned by M. Poupart, in whom the fits occurred once a week, there was "a thick, white substance, harder than jelly, under the dura mater." In a man mentioned by Lamotte, who died of an abscess of the lungs, after being many years epileptic, several sharp, bony spiculæ were found between the dura mater and pia mater, their points being directed against the pia mater. Several cases of the same kind are referred to by Van Swieten, in one of which there was an irregular piece of bone in the substance of the cerebellum, an inch long and half an inch broad. In a case by Dr Anderson, in which epilepsy came on after a severe injury on the back of the head, the posterior part of the brain, in both hemispheres, was found inflamed, and much hardened, with thickening of the membranes. A modification of epilepsy is met with, in which the convulsion is confined to one side of the body. In a case of this kind, by Dr Anderson, a portion of the brain, on the opposite side, was much indurated, with adhesion and thickening of the membranes. The same peculiarity in the symptoms occurred in Dr Beilby's case,

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* For this case I am indebted to Dr Beilby.

(case 24.) The following case, by Lieutaud, differs from these epileptic affections.

26. A man, aged 30. Had violent headach for three months, then violent convulsions, which were soon fatal. *Dissection.*—The longitudinal sinus, externally and internally, was covered with innumerable small glandular grains. Similar bodies were found on the choroid plexus. In the fourth ventricle there appeared a tumour the size of an egg, formed by a congeries of innumerable hard glandular bodies, verging to suppuration. Nearly the whole of the cerebellum had the same appearance.

SECT. V.—FIFTH CLASS.—Symptoms in the Head, with Paralysis.—Hemiplegia.

27. A man, aged 36. *Symptoms.*—Occasional attacks of severe pain in the head, shooting from behind forwards. After a few months, he found, on awaking one morning, double vision. After a few days more, his right hand was weak, and the weakness, with numbness, extended gradually over the whole right side, with distortion of the mouth, and inarticulate speech. The left eye was drawn towards the nose. Pulse natural. About two months from the commencement of the paralysis, he became convulsed. The convulsion returned at short intervals, and he died in 24 hours. *Dissection.*—On the left side of the tuber annulare there was a tumour the size of a hazel nut. It was lying on, and sunk into the tuber. It extended to the left corpus pyramidale, compressing it and the abductor nerve, and was closely attached to the basilar artery. A small coagulum was formed within the artery, at this place, and its coats were very soft. The tumour was in a state of imperfect suppuration. (Yelloly, Med. Ch. Tr. I. 181.)

28. A man, aged 63. *Symptoms.*—Six months after a severe blow on the head, had pain and sense of weight in the head, at first coming in paroxysms, then more permanent, with slight weakness of the right side, and transient loss of recollection. Symptoms increased very gradually to paralysis of right side, loss of speech, twisting of the mouth to the left side, and great failure of intellect. Died comatose more than two years from the commencement of the disease. Coma had continued a week. *Dissection.*—On the outer and anterior part of the left hemisphere there was a tumour the size of an egg, and full of blood, "which seemed to be contained in it in some places, as it is in the spleen;" in others it was in small clots, a line or more in diameter, very firm, of a cellular texture, and greyish colour, resembling tubercles. The tumour in general was of a red-brown colour, and very firm. It adhered slightly to the dura mater and the arachnoid, which was red and thickened where it covered the tumour. Below, it was imbedded in the substance of the hemisphere, and the cerebral matter was softened. ℥i. of fluid in the ventricles. (Rochoux, p. 149.)

29. A boy, aged 11. *Symptoms.*—Sudden attack of dimness of sight, amounting to blindness. It went off in a few minutes, but from that time his sight was gradually impaired, and after a year nearly lost. He then had an affection resembling chorea; and after a short time suffered an attack in which he lay speechless for three days. This was followed by hemiplegia of the right side. He complained much of his head, which appeared to his friends to enlarge: and he sometimes lost his speech for two or three days. His intellect was not affected, but at times was extremely acute. He died after coma of five weeks continuance, about a year after the attack of hemiplegia, and two years from the commencement of the disease. *Dissection.*—On the surface of the left hemisphere the membranes adhered firmly to the surface of the brain for some extent on the middle lobe. On raising them at this place fluid escaped in great quantity; and, on farther examination, it was found to have been discharged from the cyst of an immense hydatid, which was seated in the left lateral ventricle, and had gradually advanced to the circumference of the brain. It contained about ℥xvj. of limpid fluid, and, besides this, there were several ounces in the proper cavity of the ventricle. *

The cases of this class that are on record present little variety in the symptoms. They are generally headach, with or without affections of sight; and after some time weakness of the muscles of one side, which gradually increases till it amounts to paralysis. In a case of this kind by Bonetus there was a tumour three inches long attached to the side of the third sinus. Another, exactly similar, is related by Blancardus. A gentleman, mentioned by Mr Gooch, along with a variety of nervous symptoms, had an excruciating pain of one arm, beginning at the finger ends, and gradually ascending as high as the insertion of the deltoid. The arm at last became paralytic, and soon after he died in convulsion. On dissection two small encysted tumours were found on the surface of the brain, on the opposite side to the affected limb. The other parts were healthy.

Sect. VI.—Sixth Class.—Symptoms in the Head, with Paraplegia.

30. A boy, aged 7, (for whose case I am indebted to Dr Gregory,) in the beginning of the year 1811, received a violent injury of his forehead and nose by a fall. From that time he had headach. After two or three months he became near-sighted. Soon after his sight became indistinct, and after four or five months more this was followed by blindness. About this time he began to be epileptic, and affected with weakness of the lower extremities, which gradually in-

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* For this remarkable case I am indebted to Mr Headington of London

creased to perfect paraplegia. He died in April 1812, after coma of three days, his intellect having continued entire till that time. *Dissection.*—A firm, white, flat tumour, like a large bean, lay over the junction of the optic nerves. The ventricles contained ℥xii. of clear fluid. The left lobe of the cerebellum was much indurated, like scirrhus; the right lobe was reduced to a mass resembling scrofulous pus.

31. A man, aged 48. *Symptoms.*—Acute headach for a year, followed by paralysis of both lower extremities, the superior extremities being sound. Died suddenly about five months after the commencement of the paraplegia. *Dissection.*—The left lobe of the cerebellum was almost entirely scirrhous, of a pale flesh colour, and seemed to be composed of numerous small corpuscules closely compacted, without any interstice or any appearance of vessels. A small part only on the upper surface was in a healthy state. The corpus callosum, fornix, and some of the other central parts of the brain, were much softened and broken down. (Morgagni, lxii. 15.)

32. A man, aged 35. *Symptoms.*—Pain in the head, which after some time fixed chiefly in the occiput, extending down the neck. Occasional vertigo and sickness. After five months, hemiplegia of the left side; imperfect vision. Headach continued, though less severe. Hemiplegia diminished gradually; and after five or six months more he became liable to fits of stupor, which were preceded by violent pain and vertigo, and occurred sometimes twenty times in a day. Blindness of right eye, failure of memory, then paraplegia, and, a fortnight before his death, paralysis of the upper extremities also. The duration of the complaint was a year and eight months. *Dissection.*—On the surface of the pons Varolii there were two triangular fleshy tumours nearly united by their apices. The base of the one extended into the right crus cerebri, that of the other into the medulla oblongata. The disease penetrated the substance of the pons. There was much effusion under the arachnoid membrane. (Ed. Journ. XI. 470.)

33. A man, aged 63. General and complete paralysis of the whole body, which came on gradually and insensibly, without any evident cause. Speech indistinct. Mind entire to the last. Died of gangrene of the nates. *Dissection.*—A tumour the size of a hen's egg was lodged in the medullary substance of the left hemisphere, betwixt the fissure of Silvius and the part which rests upon the tentorium. It was contained in a cyst, and was internally of a brownish colour and lardy consistence. (Bull. Facult. Med. May 1816.)

SECT. VII.—SEVENTH CLASS.—Prominent Symptoms in the Digestive Organs.

34. An officer, aged 27. *Symptoms.*—Constant nausea and frequent vomiting. A slight thickening of the pericranium. Headach

and general indisposition. After a considerable time numbness of the right side. Five weeks after this died suddenly in the night. *Dissection.*—The dura mater covering the cerebellum at its posterior and inferior part was thickened and cartilaginous. This diseased state had communicated itself to that part of the cerebellum lying in contact with it, and had extended considerably into its substance. Other parts of the cerebellum softened. Membranes of the spinal cord hard and thickened. Thoracic and abdominal viscera were sound. (Med. Repos. Vol. VIII. p. 398.)

35. A medical man, in the meridian of life, had been for a year liable to attacks of dyspepsia with headach. In October 1815, he had severe headach with fever, relieved by blood-letting. After this he had various uneasy feelings which he referred to his liver; complete want of digestion, headach, and frequent vomiting, which occurred chiefly in the morning, and general emaciation. In July 1816 he visited London and Cheltenham, at both which places the first practitioners ascribed his complaints to "irregularity of the functions of the liver." In August headach increased, nothing agreed with his stomach, almost every thing was vomited. After some time the pain was much relieved, and the prominent symptoms were the morning sickness and vomiting, with increasing emaciation. Bowels torpid, frequent eructations, and hiccup. In the end of September had twice a slight convulsion. Headach periodical. Mind entire. Conversation induced headach, and sometimes convulsion. 9th October, died suddenly in convulsion. *Dissection.*—℥iv. of fluid in the ventricles of the brain. On the inferior part of the left lobe of the cerebellum, there was an encysted tumour the size of a French walnut, besides a vesicular portion connected with it containing some yellow serum. The tumour was invested both by the pia mater and dura mater, and was attached by a small pedicle to the substance of the cerebellum, where it had formed a depression in which it was imbedded. On the corresponding part of the opposite lobe there was a small florid tumour the size of a large pea. The abdominal viscera were sound. (Med. Repos. Vol. VII. p. 92.)

Many other cases are on record in which the only morbid appearances were in the head, though some of the most prominent symptoms had been in the stomach. Some of these resembled what has been called the sick-headach; others were chiefly distinguished by remarkable disturbance of the digestive functions. There is generally more or less headach, with various uneasy feelings in the head; but these symptoms are sometimes not urgent, and many of the cases have, through a great part of their progress, been referred to the digestive organs, the symptoms in the head being considered as symptomatic. A boy of 14, mentioned by Mangetus, had loss of appetite, obtuse headach, debility, and emaciation, then vomiting with more acute headach, and died after various intermissions. Three tumours were found in the brain; one in the situation of the corpora quadrigemina, and two others the size of walnuts in the substance of the brain. A young man mentioned in the Medical Observations and Inquiries, Vol. VI. had various complaints in his head and bowels which were ascribed to worms. After some time he had attacks of stupor and forgetfulness, and died delirious. The only morbid appearance was ossification of no great extent in the dura mater, with appearances of inflammation in the membranes. Similar ossification in the falx was found by Dr Lettsom (Mem. of the Med. Soc. of London Vol. III. p. 44.) as the only morbid appearance in a gentleman who had been long affected with a train of obscure complaints, the most urgent of which were obtuse headach, with frequent vomiting. On the other hand, it is to be kept in mind that similar ossifications have been met with in cases in which there existed no symptoms that could be ascribed to them. This part of the subject, therefore, is involved in great obscurity.

SECT. VIII.—EIGHTH CLASS.—Vertigo and Apoplectic Symptoms; Slight and Transient Apoplectic Attacks.

36. A man, aged 36. *Symptoms.*—After a wound on the head, which healed readily, constant headach for five years and a half; then fits of stupor, which came on at uncertain intervals, sometimes twice a-week, sometimes once a-fortnight; they generally lasted about an hour and a half; he had warning of their approach so as to lay himself down. In the intervals all the functions were natural. After seven or eight months the paroxysms became more frequent and he died suddenly in one of them. *Dissection.*—A scrofulous tumour, larger than a hen's egg, lay in the middle of the left hemisphere of the brain, extending, in depth, to nearly on a line with the corpus callosum. It seemed to be merely a part of the brain in an indurated state. A piece of bone, the size of the finger nail, was attached to the left side of the longitudinal sinus. The veins on the left hemisphere were more distended with blood than those on the right. (Med. Ch. Tr. IV. 188.)

37. A woman, aged 28. *Symptoms.*—Severe headach, constant vertigo, nausea, occasional vomiting, frequent rigors, pain and deafness of the left ear, and the left eye somewhat affected. After several months the headach increased, with occasional paroxysms of coma, and she died at last rather suddenly, having been for a day or two affected with extensive erysipelas of the head and face. *Dissection.*—A remarkable tumour under the base of the brain, on the left side; it consisted of three portions; the anterior was the size of an egg, of a pink colour, and composed of a spongy vascular substance, like the texture of the placenta, interspersed with small cysts, containing puriform fluid; the posterior portion was half the size of the former, and nearly similar in structure, but firmer; the middle portion was the size of a walnut, of a white colour, and nearly cartilaginous structure. On the petrous portion of the temporal bone, where

the tumour rested, the osseous substance was absorbed to the depth of half an inch.

38. A lady, aged 64. *Symptoms.*—Attacks of headach, giddiness and imperfect vision, occurring at uncertain intervals; afterwards nausea; at last, after five years, death, with fever and delirium. *Dissection.*—Two small aneurisms, each about 5-8ths of an inch in diameter, formed by dilatation of the internal carotid arteries, by the side of the sella turcica, and containing laminæ of coagulated blood. (Trans. of Soc. II. 193.)

39. A man, aged 20. *Symptoms.*—Feeling of weight in the head, frequent attacks of vertigo, followed by great weakness, prickling of the left arm, with coldness and wasting of it, and the pulse in that arm was much weaker than in the other. Found dead in bed. *Dissection.*—Brain indurated in several places; ventricles full of a puriform fluid; cerebellum covered with a cartilaginous concretion, the thickness of a six-livre piece. (Portal sur l'Apop. p. 144.)

40. A man, aged 73, (for whose case I am indebted to Dr Hunter,) about six months before his death, began to be affected with headach, and occasional attacks of giddiness and loss of recollection. He did not generally fall down, but sometimes continued walking without knowing whither he was going; at other times the attack resembled intoxication. He generally recovered his recollection in a short time. His gait was feeble and tottering, and the attacks gradually increased in frequency and in violence. At length, after one of them, more severe than usual, he was confined to bed with violent pain of the forehead, giddiness, and loss of memory; pulse 78. Bloodletting, purgatives, blistering, &c. were employed without relief; the fixed pain of the forehead continued; he became restless and incoherent, and about the 12th day from his confinement, was affected with severe pain, and partial paralysis of the left arm and leg. After three days more he had general and severe convulsion; he then lay for six days in a state of increasing weakness, but able to answer questions distinctly. After this the convulsion returned with perfect hemiplegia and coma, and he died in two days. *Dissection.*—Membranes of the brain very vascular. ℥iij. of fluid in the ventricles; cerebral substance very firm. In the posterior lobe of the right hemisphere, there was a firm tumour the size of a small pigeon's egg; internally, of a pale flesh colour, and granular texture. It was not inclosed in a distinct capsule, but for a considerable part of its circumference it was covered by a texture resembling the fibrous bands of carcinoma. The tumour communicated with the ventricle, so as to form part of the wall of the posterior and inferior cornua, and the margin of the pes hippocampi, where it is about to descend into the inferior cornu, was attached to it. The pia mater lining the ventricle was at this place very vascular.

Many cases are on record which belong to this class, and the principal difference that they exhibit in the symptoms is, that in some of them we find constant uneasiness, with vertigo, resembling the apoplectic tendency, in others frequent and transient apoplectic attacks, the patient, in the intervals, sometimes enjoying tolerable health, in other cases complaining of a slight and habitual giddiness, commonly called weakness of the head, often accompanied by unsteadiness of the limbs. In a case of the first kind, by Zeder, he found numerous hydatids in the brain, one, particularly, in the passage to the aqueduct of Silvius, larger than a hen's egg, and containing within it three smaller hydatids. Of the other class of cases, distinguished by frequent and transient apoplectic attacks, I have formerly given a remarkable example, in which there occurred a tumour formed by thickening of the dura mater; and I have quoted, from Lancisius, the case of a gentleman who had an apoplectic attack once or twice every month, connected with thickening of the membranes, and a polypous tumour under the frontal bone. Various other modifications occur of this form of the disease. A gentleman mentioned by Gooch had been for several years liable to attacks, in which there was sometimes a shaking of the head, and a kind of emprosthotonos, at other times he became vertiginous, and fell down deprived of sense for a short time. He was never entirely free from headach, and brisk exercise excited giddiness, which went off immediately upon resting. He died suddenly in convulsion, and on dissection there were found several osseous points arising from the right parietal and occipital bones, and irritating the dura mater, which was inflamed and beginning to mortify. (Gooch's Appendix, p. 237.)

To this class also belong the cases in which, connected with organic disease in the brain, there has occurred a gradual abolition of the mental faculties, with little complaint of pain, or any other urgent symptom. In a case of this kind, which terminated in perfect stupidity or lethargy, Platerus found a firm fleshy-looking tumour the size of a moderate apple, above the corpus callosum. (Plateri, Obs. Lib. I. p. 13.)

To this very imperfect view of the important subject of organic disease of the brain, I shall only add a tabular view of the cases which have been either described or referred to. From this it will appear that it is difficult or impossible to fix upon any general principles, or to refer the particular character of the symptoms to any thing in the nature or seat of the disease, and that the whole subject is still involved in much obscurity. Tumours, for example, will be found under the first class, some of them of large size, unaccompanied by any remarkable symptom; while tumours in the same situation, under the other classes, were accompanied by blindness, convulsions, or paralysis. It does not appear, that these diversities depend upon the size of the tumours, or, as far as we know, upon these particular structure; but these points remain to be investigated, particularly what diversity of symptoms are connected with the nature of the tumours, and especially with their characters, as being tumours distinct from the cerebral mass, or as being indurations of part of the brain itself.

Many of these cases seem to illustrate the doctrine which I formerly proposed in regard to pressure on the brain. I have contended, that causes which are supposed to act in this manner, produce their effects, not by compression of the cerebral matter, but by their influence upon the blood-vessels of the brain, in destroying that relation betwixt the arteries and veins of the brain, which, in an organ closely confined in a cavity of bone, must be essential to a healthy state of the circulation. I have endeavoured to trace the manner in which compressing causes may exist in a very considerable degree, without thus affecting the circulation, and, consequently, without producing coma; and I have mentioned cases, which I think could only be explained on this principle, in which extensive effusion existed in the brain, without coma. I see no other principle on which we can account for many of the cases of organic disease. In some of them, tumours of a great size were fatal, without coma; and in many more, though coma existed for a short time before death, there was every reason to believe that it was connected with a recent inflammatory action, and did not depend immediately on the original disease.

In regard to the treatment of these diseases, there is little to be said. I am persuaded, however, that we ought not to consider them all as hopeless. Many of them certainly have their origin in inflammatory action, and, by the proper treatment, we have every reason to suppose that their progress may at least be impeded, and the life of the patient prolonged, as well as rendered more comfortable. This treatment will consist in keeping the system extremely low by evacuations and spare diet, cold applications to the head, issues or setons in the neck, and avoiding all causes of excitement.

TABLE OF CASES OF ORGANIC DISEASE OF THE BRAIN.

Prominent Symptoms.	Morbid Appearances.
Long continued headach, without any other urgent symptom.	Tumour, five inches in circumference, attached to the falx.
	——— the size of a walnut, behind the medulla oblongata.
	——— the size of a pigeon's egg, at the upper and middle part of right hemisphere.
	——— three inches broad, adhering to the membranes, by the side of the third sinus.
	——— hard, irregular, and of a stony consistence, near the torcular.
	——— stony, like a mulberry, in substance of the brain.
	——— the size of a nutmeg, in the seat of the pineal gland.
	——— the size of a pigeon's egg, in the substance of the cerebellum.
	Small triangular ossification in the falx.
Headach. Blindness.	Tumour, the size of an egg, attached to the anterior part of tentorium.
	——— the size of an egg, between the optic nerves.
	——— the size of a large walnut on lower and anterior part of left hemisphere.
	——— larger than an egg, compressing the optic nerves.
	——— the size of a fist between the brain and cerebellum.
	Pineal gland enlarged to the size of an egg, and of an earthy consistence.
	Cyst in left hemisphere, containing ℥ij. of pure albuminous fluid.
Headach. Blindness. Convulsion.	Tumour, the size of a walnut, on the sella turcica.
	——— the size of a small orange, on petrous portion of left temporal bone.
	——— the size of a bean, on left side of pons Varolii.
	——— osseous, three inches broad, on inner surface of left parietal bone.
	——— larger than an egg, in the substance of left hemisphere.
	——— two inches long, in posterior lobe of left hemisphere.
	Four hard tubercles, each an inch in diameter, in right hemisphere, and one in right.
	Cyst like a hydatid, three inches long, in substance of right hemisphere.
Headach. Convulsion. Loss of Smell.	Tumour in the substance of anterior part of right hemisphere.
Headach. Convulsion.	Tumour, the size of a nut in the left lobe of the cerebellum.
	——— the size of an egg, in the fourth ventricle.

Prominent Symptoms.	Morbid Appearances.
Epilepsy.	Tumour of the dura mater, projecting outwards through an opening in the bone.
	Hydatid, the size of a pigeon's egg, in posterior part of the right hemisphere.
	Induration of pons Varolii.
	——— of posterior part of both hemispheres.
	Three tubercles in dura mater, on right side, and one in right hemisphere.
	Seven sarcomatous tumours near longitudinal sinus, and one in corpus striatum.
	Firm gelatinous substance under dura mater.
	Bony spiculæ between the dura mater and pia mater.
	Irregular piece of bone in cerebellum, one inch long, and half an inch broad.
— the convulsion confined to the right side of the body.	Tumour, the size of an egg, on surface of left hemisphere.
	Induration of part of left hemisphere, with thickening of dura mater.
Head symptoms, with Hemiplegia.	Tumour, the size of a hazel nut on left side of tuber annulare.
	——— the size of an egg, on surface of anterior part of left hemisphere.
	——— three inches long, attached to the third sinus.
	Two encysted tumours on surface of brain, (palsy of the arm only.)
	Large hydatid in the left ventricle.
Do. with Paraplegia.	Induration of left lobe of cerebellum, and suppuration of right.
	Scirrhus of left lobe of cerebellum.
	Tumour, the size of an egg, in left hemisphere, (universal paralysis.)
	Two tumours on the pons Varolii.
Stomach remarkably affected.	Induration of surface of cerebellum, with thickening of membranes.
	Tumour the size of a walnut, under the cerebellum.
	——— in seat of corpora quadrigemina, and two in substance of brain.
	Ossification in dura mater.
	Ditto in falx.
Comatose or apoplectic attacks.	Tumour, larger than an egg, in left hemisphere.
	——— remarkable lobulated, under base of brain, on the left side.
	——— five inches long, from thickening of dura mater, on top of right hemisphere.
	——— size of a pigeon's egg in posterior part of right hemisphere.
	——— Polypous under the frontal bone.
	Two small aneurisms of internal carotid.
	Induration of brain in various places.
	Numerous hydatids in the brain.
	Osseous projections from the inner surface of the occipital and right parietal bones.
	Tumour, the size of an apple, above the corpus callosum, (gradual loss of intellect.)

II.

Case of Abscess of the Lungs, cured by Paracentesis Thoracis.

By George Hawthorn, M. D.

William Longmoor of Clay, near Banbridge, county of Down, Ireland, pensioner, by trade a linen weaver, in the 34th year of his age, of a robust and full habit of body, rather given to intemperance.

On the 2d of November 1817, (in consequence of exposure to damp night air, in a state of intoxication, after being previously much heated,) was seized with an acute pneumonic inflammation, which increased in severity until the evening of the 6th, before I had an opportunity of seeing him; during which time, nothing had been used for his relief, but, as a favourite nostrum among the vulgar, a large quantity of ardent spirits had been given, as they said, with the view of putting him into a sweat, which obviously had the effect of aggravating every symptom.

He complained of obtuse deep-seated pain in his left side, extending towards the sternum, great oppression, and difficulty of breathing, approaching to orthopnea, with cold shiverings at times. Face swelled and livid; very severe dry cough; delirium at times; pulse 120, soft and feeble; heat of skin very great, and much thirst. In fact, every concomitant symptom exhibited the most alarming appearance.

Notwithstanding the most active treatment, bloodletting to the extraordinary amount of 196 ounces in four days, and every other remedy from which any benefit was likely to accrue, the disease, with intermissions, went on for three weeks, when it terminated in a most extensive suppuration, as will appear from the following history.

He could now breathe only in the sitting posture, and rather leaning forwards; pulse 130, feeble and irregular. He complained of great weight and oppression in the side affected, with cold shiverings, profuse, cold, and clammy night sweats breaking out about his neck and shoulders, coldness of the extremities, great prostration of strength, incessant cough, and sense of suffocation; and, upon examination by applying the hand over the part, a distinct fluctuating undulation could be perceived.

From the severity of the preceding symptoms, and great oppression and sense of suffocation which the patient laboured under at the time, I had no doubt that a large quantity of matter was formed, and dreaded the usual fatal consequence of the rupture of the abscess.

And my fears were much increased by the result of an unfortunate case, which a short time before came under my observation. of a woman labouring under the same disease, in whom all the symptoms bore a striking similarity to the case I am now describing, and where I in vain urged the propriety of having recourse to an operation as a last resource.

This case, therefore, indicating the same necessity, I explained to his friends his danger, the propriety of the operation, and the consequences which might result from neglecting it; but all my expostulations had no effect. However, he was soon partially relieved by an immense effusion of purulent matter into the bronchial cells, which was expectorated by coughing in great quantities, to the very extraordinary amount of 5 lb. or 6 lb. in the day, and of an intolerably fetid putrid smell and appearance There also issued from his whole body a most offensive, putrid, cadaverous smell. During which time, every means and remedy were used which were likely to contribute to his relief.

His feet and legs now began to swell, which was soon succeeded by an anasarcous state of his whole body, combined with ascites. These affections were partly removed by the use of diuretics and appropriate remedies.

He remained in this deplorable state, every symptom becoming more aggravated, and the vital powers more and more exhausted, until the expiration of nine weeks from the suppuration being established.

The 28th of January 1818, being twelve weeks from the first attack, the purulent expectoration, which had continued, was now become immense; his feet and legs much swelled and cold; features ghastly; cold profuse night-sweats, particularly about his neck and shoulders, with colliquative diarrhœa; great prostration of strength; sense of weight and oppression about the breast, and more particularly in the side affected, with sense of suffocation. His breathing was now still more laborious and imperfect, from oppression and debility, and his articulation very difficult.

His impending fate now became obvious to every one, and I at length succeeded in obtaining permission to perform the operation of paracentesis thoracis.

Having, therefore, made the necessary preparations, I placed the patient in a proper posture, supported by one of his friends, being first satisfied as to the situation of the matter, and, without an assistant, commenced the operation by holding up the integuments in order to form a valve over the internal orifice. I began my first incision with a scalpel between the sixth and seventh rib, about six inches from the sternum, extending it about two and a half or three inches in length, cutting as near to the superior margin of the inferior rib as possible, avoiding the danger of wounding the artery, vein, or nerve running in the groove of the inferior margin of the rib above, carrying my first incision through the skin and cellular substance, dissecting the intercostal muscles cautiously The pleura now being exposed, with the point of an abscess lancet, I divided it freely with the sac of the abscess, when, upon the instrument entering the abscess, the patient gave a great cough, and above half a pound of matter gushed up his throat, while his breath issued forth from his side with a great blast, dashing out about my hand, and round the room, matter of a most abominably fetid putrid smell and appearance. About 12 lb. of vitiated matter were discharged; and, contrary to what is supposed by some, instead of becoming faintish or sinking from the sudden removal of the pressure, he became always stronger in proportion to the discharge, and exclaimed he was quite relieved, which, I believe, is generally the case.

The valve was now let down over the orifice, in order to exclude the excess of the air. A soft, warm cataplasm was applied over the wound, and the patient turned to lie on the same side (to favour the exit of the matter) to which he had not before reclined from the first attack.

He now felt quite relieved, and rested well during the night; and his friends assured me that above 10 lb. more were discharged during the night. Upon removing the cataplasm next day, about 4 lb. were discharged; and every day, for eight or ten days, about 2 lb. were discharged; after which, the discharge gradually decreased, until the wound healed up altogether, which was about six weeks from the operation. During this time, nothing was neglected which the critical nature of his case seemed to require. At every dressing, while the wound remained open, his breath issued forth from his side during expiration with a whistling noise, and with considerable force, sufficient to blow out a candle at some distance.

The expectoration, which had been so profuse previous to the operation, immediately disappeared on its being performed, and never returned; unless occasionally when the orifice ceased to discharge for a day, or past the usual time; but it quickly disappeared on the discharge being re-established; proving the great utility of the operation, and indicating the free and extensive communication between the external orifice and the bronchia.

He now recovered health and strength rapidly; so that, in three months, he was quite well, and declared he never enjoyed better health in his life, only that he was a little more easily fatigued by exercise. He then resumed his trade as a weaver, and has since enjoyed good health, perfectly free from all complaints.

I have thus given a very brief and cursory outline of the particulars of this very extraordinary case, which is a striking instance of the utility of active treatment, and the impropriety of resigning the unhappy sufferer to his fate, although in circumstances seemingly very unfavourable. It is also evident, that if the operation had been performed when first proposed, the case would have been much more favourable, and with much less risk of disappointment.

It affords, also, a strong presumption that it would be likely to succeed in many more cases than is generally supposed, providing it be performed in proper time, and in the proper manner. The recovery of the patient in the case above described, after such an extensive ulceration and destruction of the substance of the lungs which must have taken place, must, indeed, by every one be acknowledged as very extraordinary, considering such a large mass of vitiated matter so long in contact with such irritable and important organs as the lungs; which likewise affords the strongest presumption, that, when this case succeeded in such very unfavourable circumstances, in one so far exhausted, so long labouring under such profuse colliquative discharges, and all the destructive devastations of hectic fever, our hopes may be sanguine in those cases where few or none of those untoward circumstances may have occurred.

We are reminded by all writers on inflammation of the important necessity of evacuating, as early as possible, all collections of matter in the vicinity of large joints, blood-vessels, or important organs. Then, surely, of all the organs of the body, there are none of more importance than the lungs and thoracic viscera. Therefore, I think it is our duty, in every case where it is in any wise practicable, to have recourse to the operation.

Banbridge, County of Down, Ireland.

III.

On Pemphigus. By W. Ogilvie Porter, M.D. One of the Physicians to the Bristol Dispensary, &c.

It was an error of the older Nosologists to confound accidental with essential appearances in fever, by which the occasional exanthematous occurrences in typhoid fevers were indiscriminately intermixed with the diagnostic eruptions of the contagious exanthemata. Thus Sauvages places Pestis as the first genus of his order "*Exanthematicæ.*" And why? Because it exhibits an eruption of buboes and carbuncles; he might have added, of bullæ, phlyctenæ, maculæ, vibices, and petechiæ, separately or combined. He then jumbles together small-pox and this pemphigus, measles and the miliary eruption, scarlatina and the purplish maculæ, &c. making each a distinct genus; thus confounding all distinction between the essential and the accidental in disease. Sagar attempted an improvement on this arrangement by dividing his "*Exanthemata*" into two orders, "*Contagiosa*" and "*Non-contagiosa;*" but this very arrangement induced him to class the exanthematous appearances of typhus and typhoid fevers with variola, rubeola, and scarlatina. It must be acknowledged there is some difficulty in this matter; but nosologists will not escape criticism who take up an occasional symptom of any disease, however imposing, and erect it into a genus, or into any thing more but what it is.

Pemphigus has, however, been considered a peculiar febrile disease, claiming a distinct place in our nosologies, and as such has been received and distinguished by systematic writers up to the present day; but its history has always varied so much, that, excepting in the one circumstance of the appearance of the bullæ, the precise nature and extent of the fever could not be determined upon: its very existence was doubted by the celebrated Cullen, on account of the discordant histories which had been related of its rare and singular appearance. "Cum omnia fere de eo dicta, dubia, obscura et ambigua mihi videntur;" says he. At length he saw one case of fever attended by bullæ. "Paulo postquam hæc impressa sunt," continues he, "collega noster, eximius Franciscus Home mihi hominem leviter febriculantem ostendit; cui primum in brachiis, et successive demum in toto corpore, vesiculæ magnitudinis avellanæ abortæ sunt, et post duos tresve dies, effuso humoris serosi pauxillo collapsæ sunt. Hæc febris autem nullam indolem, vel typum peculiarem monstrabit, et cito dis-

paruit, nequaquam contagiosa." Yet, notwithstanding these doubts, and the evidence set before him, he follows preceding nosologists, and makes *pemphigus* a peculiar fever, placing it in his order "*exanthemata*," and assumes the type to be "*typhus contagiosa.*" Dr Young adopts a different arrangement, placing it directly as the sixth variety of "*typhous fever*;" but, in so doing, he expresses a doubt as to its deserving a place, by annexing a note of interrogation. This appears to be the most judicious disposal of the subject hitherto adopted: it was either to be considered a variety of "typhous fever," upon the evidence of all who had seen the disease; or it was not a specific fever belonging to any genus whatever. He did not follow the older nosologists, seeing that the bullæ do not observe similar laws to the eruptions of truly exanthematous fevers. Mr Good, however, in his late very learned work, has restored this appearance to its former station among the "*exanthematica*," "*eruptive fevers*;" placing it in his second genus "*emphlysis*," which he defines, "An eruption of vesicles, whether large or small, produced by, or accompanied with, internal and febrile affection, essentially connected with it." He makes *pemphigus* the fifth species of this genus, and gives of it the following definition: "Vesicles scattered over the body; transparent; filbert-sized; with a red inflamed edge, but without surrounding blush or tumefaction; on breaking, disposed to ulcerate; fluid pellucid, or slightly coloured; fever, a typhus."

Having taken notice of these standard nosological writers, in reference to the disease in question, I shall quote the mention of its appearance by a few authors, who have respectively seen it under various circumstances, and add a case of it that presented itself lately to my own observations; and hence endeavour to come to some conclusion as to its real nature.

Darwin says, in his Zoonomia, "Pemphigus is a contagious disease, attended with bladdery eruptions, appearing on the second or third day, as large as filberts, which remain many days, and then effuse a thin ichor." He had seen only two cases of it.

Dr Withers, in a work on asthma, published many years ago, relates several cases of fever, with large bladdery eruptions; some cases of which were mild; in others "the fever was extremely putrid, with many gangrenous sloughs, of a fetid smell and most dreadful appearance, following the eruption of the blisters, attended with a low delirium for many nights; the greatest dejection of countenance, and prostration of strength."

Dr Fuller, who wrote about a century ago, takes notice of this disease in his "*Exanthematologia*," under the head of "*crystals*." "These come," says he, "with the very same headache, back-ache, vomiting fever, and other symptoms usual in a true small-pox; but sometimes, without any foregoing illness at all, they break out, the third or fourth day, in certain distinct bladdery pustules, dispersed all over the body, plump and shining, like crystals, from whence the name; which, if pricked, send forth clear water, and no pus."

Dr Russel, in his treatise "Of the Plague," describes a variety of carbuncle in these words: "The cuticle was raised at once into a blister of the size of a horse bean, half filled with a dusky yellow or blackish fluid, round which the skin was less tense, and of a paler red, than the others." He then proceeds to observe, in a note, "This I conceive to be the fourth species of *gotwald*, and what Hodges terms blains, describing them as "vesications of size from a pea to a nutmeg, encompassed with a variegated circle, generally reddish."

Dr Steward gives a case, in Duncan's Medical Commentaries, of a soldier being attacked by a fever, which exhibited these bullæ a few days after the premature disappearance of the rubeolous eruption from exposure to cold. He did not communicate the fever to any one.

Dr Dickson, one of the physicians to the fleet, and now a resident physician at Clifton, has given four very interesting cases of this appearance in the 10th volume of the Edinburgh Medical and Surgical Journal, with many judicious remarks. The first case occurred with mild fever; "the vesicles were of every size, from a pea to that of a walnut, irregularly circumscribed, but generally oblong, and filled with a straw coloured transparent fluid;" "the febrile symptoms were mild, and the functions nearly natural." Every part of the body produced some of these vesications, and there was a succession of fresh ones from time to time for some days. This patient had a symptom in common with my patient, whose case I am about to detail, that has not been noticed by other reporters,—"The tunica conjunctiva is much inflamed, and the exposure to light causes a copious flow of scalding tears." The second case related by Dr Dickson arose in a convalescent Russian sailor. The vesications continued to appear on every part of the body during a week, and produced such a degree of irritation as finally to exhaust the powers of life; and he died within two months of their first invasion. This patient, too, manifested severe ophthalmia during the presence of the bullæ. The third case was less interesting. But the fourth shews that these appearances are not the legitimate product of a specific fever; they occurred abundantly after the night-sweats of a patient labouring under phthisis pulmonalis, and continued to make their appearance during three weeks. No notice is taken of the state of the eyes, which I regret; but, as the doctor did not see the case himself, it is not impossible that ophthalmia may have been overlooked by the gentleman who reported it.

I will now transcribe the case which lately came under my own observation, and then close this paper with some opinions, and my own, respecting the nature of these bullæ.

Case.—*Pemphigus*.—September 17th, 1818.

Richard Stokes, æt. 4, of previous ordinary health and appearance, having had rubeola and vaccina, but not scarlatina or pertussis, complained of chilliness and headache. Towards the evening of the next day, these symptoms continuing, with aversion to food, a spontaneous vomiting took place; and that night the skin became hot, and he was said to be slightly delirious.—I saw him on the 19th. Complains of head; skin hot and dry; face a little flushed; eyes somewhat red and watery; tongue furred towards the root; pulse 120, small, rather tense. Six leeches to be applied behind the ears, and two grains of calomel and five of jalap to be given every fourth hour.—20th. Bowels well, and copiously evacuated; pyrexia unabated; head bewildered, not painful; eyes rather red; face a little flushed; tongue nearly covered with a brownish fur; pulse 120, small, not at all tense. Continuentur pulveres cathartici.—21st. Many small greenish stools; complains much of his bowels; head much bewildered, rambles in his talk; skin less hot; face not flushed; eyes as in measles; tongue much furred; great thirst; pulse 127. Nuchæ emplastrum lyttæ imponatur. A simple saline mixture is prescribed.—22d. Head perfectly relieved, no delirium; other symptoms unabated. Mistura salina continuetur, et balneum tepidum vespere utendum.—23d, 24th, 25th. Little amendment; the bowels are easy, the stools pretty free and natural; the fever remits a little every morning, and increases with the advancing day, which has been part of its character from the commencement.—26th. Complains much of his throat; makes a stridulous noise like that of croup; screams frequently; does not complain of his head or bowels, but has again been slightly delirious; no stupor; tongue furred, and covered with a tenacious mucus; skin hot and dry; eyes more red; face irregularly flushed; bowels open, stools thin and yellowish; pulse 130, small, not feeble.

> ℞ Hydrargyri submuriatis ʒss. Pulveris ipecacuanæ compositi gr. xij. Sacchari albi ℈i. Fiat pulvis in chartas sex dividendus. Sumat unam sextis horis.

27th.—Affection of the throat worse, but the respiration does not make a noise to-day so much resembling croup; he puts his fingers into his mouth, he says, to pull something out; he is very irritable, will not suffer the throat to be inspected, or himself to be touched. Continuentur pulveres.—28th. Several copious spinage-looking stools, and much griping; he screams frequently on account of the pain in his bowels; very little headache; febrile heat unabated; tongue appears aphthous, can only see its apex; no mercurial fetor; much distressed by the state of his throat; pulse 130, small, and rather weak. Discover a *bulla*, the size of a duck's egg, on the fore arm; it is a transparent vesication, surrounded by a narrow margin of rose colour; there is not any increase of heat in the part, nor circumjacent tumefaction. In the course of the day, two other bullæ, of the size of walnuts, rise over the sacrum, and several smaller ones on the legs and thighs. The boy's mother says she watched the rise and progress of one of them. It first appeared like a pin's head, and in two hours attained its full size; the one she observed did not exceed the size of a filbert. Ordered the bullæ to be punctured with a needle, and two grains of compound powder of ipecacuan, with a few grains of prepared chalk every four hours.—29th. The bases of the bullæ on the fore-arm and sacrum appear gangrenous; several small ones have arisen on the limbs since yesterday; two greenish stools; does not complain of the bowels; no headache; sleeps much; no stupor; slight delirium; urine pale, and has been so throughout the disease; pulse 120, weak. Continuenter pulveres; wine and water; poultices to the bases of the large bullæ.—30th. One stool; febrile symptoms unabated; nearly choked by something in his throat, most likely a bulla, or its remains. Habeat antimonii tartarisati gr. iij. pro emetico.—October 1st. The tartarised antimony did not vomit him, but brought away a copious brown motion of some consistence; less fever; pulse 112; looks lively; small sloughs separating at their edges from the bases of the large bullæ; a great deal of nasty mucus in the mouth; tongue foul and sore; no fetor; the obstruction of the throat much relieved. Wine and water, and light nourishment.—2d. Rather better.—3d. Better; the bases of the three large bullæ look well; the sloughs have separated, leaving an ulcerated surface of the size of an old shilling in the largest, but not reaching deeper than the cutis vera, which had been destroyed; the sites of the other bullæ appear purplish, some of a lilac colour; the febrile symptoms continue; there is much irritation. Omit the wine and water.

> ℞ Pulveris ipecacuanæ gr. iij. Pulveris ipecacuanæ compositi gr. ix. Sacchari albi ℈i. Fiat pulvis in chartas vi. dividendus. Sumat unam quartis horis.

4th.—Bowels open; slept well; is quiet, excepting when disturbed by a slight cough; throat very much better; he spat up some bloody mucus this morning; less febrile heat; tongue cleaner; pulse 120, more open; ulcerated surfaces of bullæ look well. From this day the progress to recovery was uninterruptedly progressive; he was much emaciated; he recovered slowly; his skin desquamated, and his hair fell off; he was not able to walk across the room until the middle of the month.

The case I have detailed varies little from other specimens of the fever which for some time back has prevailed in this city, among children and young people, if we except the appearance of the bullæ, the affection of the throat, and ophthalmia, which are linked together in morbid connection, under the operation of the unknown irritative cause that produced them. If this case stood alone on the records of medicine, it would tempt us to conclude, that it presented us with a correct view of a strange but true exanthematous fever; having the eye of measles, with the throat of scarlatina, and a peculiar eruption of large vesications on the eleventh day. This reflection should induce us to record much, and generalize little; for I believe no two parallel cases of pemphigus have yet occurred. The fever pourtrayed in the case above detailed, is certainly of typhous alliance, notwithstanding the mildness of its character; and I consider it a case of the prevailing fever; in which, where several persons of one house are affected, the younger branches evince only slight typhoid symptoms, while the elder ones suffer under a fever of unequivocal oppression. However, there has not been any other person ill in the family where this case occurred; and the sporadic nature of the prevalent fever, opposes the notion of its contagious propagation. Solitary instances present themselves in every direction; but only a few examples of three or four ill in one house have occurred; yet such cases do not become the centres of extension. I do not mean to say, that an individual labouring under this fever, is incapable of propagating it by contagion; I believe any case of it might be converted into a hot-bed of that morbific influence, by confining the patient in a small close room, without fire-place or opening window, and covering him up with blankets, under the operation of diffusive stimuli. It was in this way, I believe, sporadic fevers formerly became contagious, and hence epidemic. The improvement of civilized life, and of medical science in the present day, preserves communities from the ravages of general contagion; but sporadic fevers seem to depend on the nature of the seasons; and if so, mankind must ever be liable to their occasional appearance, and to their epidemical extension, from a greater intensity of the same cause, even without the intervention of contagion.

But it were waste of time to inquire into the nature of the fever my patient laboured under, which gave rise to these bullæ; for they have appeared in every grade of fever, from febricula to the plague, and where there has been no fever at all; therefore, to say *pemphigus* is, or is not a *contagious fever*, were to confound the accident with the thing, the misletoe with the oak. But if I should be mistaken in the opinion I venture to entertain respecting the accidental nature of the appearance above alluded to, the case I have detailed will serve, beyond all others I have met with, to establish the view already taken of the disease by every nosologist up to the present day, with the exception of *Dr Thomas Young.*

But it is so certain that these bullæ appear without fever, that *Dr Willan* seeing this, felt himself under the necessity of creating a new genus for them, under the designation of *Pompholyx;* and in this he is followed by *Parr*, *Young*, *Bateman*, and *Good.* And *Dr Dickson* is of opinion, that bullæ, under all circumstances, should range themselves in varieties under this head.

I must not close this paper, without adverting to the opinion of *Dr Bateman* on this subject. In his Synopsis of Cutaneous Diseases, he takes a cursory view of the reports respecting the appearance of these bullæ; and finds himself obliged to conclude, that they and all the smaller phlyctenæ which are referable to typhoid fevers, are accidental and sympathetic. This opinion, and the opinion of Dr Dickson, and the opinion implied by Dr Young in rejecting *pemphigus* from the exanthemata, perfectly coincide with my views of the subject. I believe the bullæ, in all cases, to be the offspring of various irritations:—they have succeeded to the taking of certain substances into the stomach; they have attended dentition; accompanied a hot season; the night-sweats of phthisis; they have broke forth under the irritation of suppressed measles; they have appeared in febricula, in typhus, and in the plague. *Pemphigus*, then, is not a specific fever, but is a name given to any fever in the course of which bullæ may exhibit themselves.

Bristol, October 23, 1818.

IV.

Memoir on the Extraction of a Tooth. By G. N. Hill.

J. Stretch, æt. 52, a very athletic man of the class of hard labourers, applied at the surgery of an hospital, to have an incisor tooth extracted. It was not (apparently) in a state of decay, but had ached much for some weeks The extraction was perfect, but attended with most severe pain, which did not abate in the usual time or manner; but the socket and neighbouring parts continued uneasy for some weeks. At length every thing seemed right. Two months after the operation, he perceived a soreness in the roof of his mouth, when touched by his tongue, or a crust, with a fulness and softness extending from thence to the spot from whence the tooth had been extracted. Soon afterwards he discovered a puffiness, originating in the empty alveolæ, and occupying the usual cavity formed in the gum by the loss of a tooth, situated on the right side of the frænum of the upper lip, and the size and shape of a very small mushroom. It soon became disposed to bleed on the slightest touch; was of a dark mulberry colour, but not painful. It rapidly increased in growth, and was evidently connected by a line of puffiness, with the palatine tumour first noticed. Both combined to affect his speech, which now began to be thick, and troublesome to understand. In this state he applied again to the hospital; when the surgeon of the week, on a cursory inspection, told him he had been "smoking with a foul pipe," he must have some pills to take every night, and use a wash for his mouth. These steps were taken, and pursued for several weeks, but made no alteration in the state of the mouth of the sufferer, at the end of three months. December 13, 1815, he called upon me, saying he had a bad sore mouth, with a swelling on the right side of his neck, just below the angle of the jaw, which his barber had that morning discovered. On examination, I found that this tumour seemed to be an enlarged gland, the size of a pullet's egg, loose and painless; the mouth presented a spongy, bleeding, fungous swelling, protruding the upper lip, and extending backwards to the centre of the ossa palati, terminating abruptly in a sort of indentation. The teeth on each side the tumour were loose and divergent, appearing as though stuck in a firm jelly. The slightest handling of these parts produced a discharge of venous blood. I removed the loosened teeth, each of which brought away with it a large piece of fungus, with a scalpel. I also removed the whole of this substance as clear as I could. Much blood flowed, continuing to do so for some hours. The next day the bones of the maxilla and palate were carefully examined; but I could not discover any undue roughness or softness; a soap liniment was directed for the swelled gland, with a covering of oil-cloth and flannel; the mouth was directed to be frequently washed with a solution of sulph. cup. made as strong as he could bear it. He complained of want of appetite, sickishness, sense of feebleness, (though a powerful man,) a short cough, and propensity to costiveness. After a mild aperient, he took the nitric acid in a bitter infusion, during the day-time, and at night, pills of extract. conii, pulv. antim. Lond. and a little opium. At the end of three weeks, he said his general health was much better, but the fungus was again rising in the former direction. It was now rubbed down daily with argent. nitrat; but when the sloughs occasioned by this application came away, the subjacent parts shewed no improvement. The stronger caustic kept all down for some time; but this in its turn ceased to be serviceable. My patient then consented to have that part of the maxilla removed, whence the tooth had been drawn, and where the fungus appeared to sprout from. I freed the lip from the gum, removed the latter from the subjacent bone, to as great an extent laterally and backwards, as to enable me to saw into the firm parts with one of Mr Hey's circular saws. I removed all the parts from whence the disease seemed to spring in front, and would have carried the operation as far back as possible, had not the fatigue, loss of blood, and distress of the poor man, compelled me to desist. In a week afterwards, all the parts looked very well, and in a healing state; those which had not been removed shewed no marks of disease, more than being somewhat softer than common; the tumour beneath the jaw evidently increased, had an elastic feel, but was easy and moveable. A month after this operation, the tooth adjoining that part of the maxilla which had been sawn away became loose, its gum flabby, giving an exact resemblance to the former ones; I took it away, and cut out all the pappy gum; the part of the palate corresponding with this tooth, assumed the same brown flabby appearance as before, while nothing could look better than the opposite spot, which had suffered so much. The new place soon threw out the same soft bleeding fungus as before, resisting the action of every escharotic I could devise. My patient grew so disgusted with their use, that he requested me to have recourse in future to the knife; with this instrument every softened diseased part which I could reach, was removed on the 31st March. The tumour was now larger, more fixed, the countenance of the patient sallow; he was also thinner, but said he felt well. In defiance of every attention, the employment of the scalpel was as necessary on the 28th April as it had ever been before. At this period, on minutely inspecting the throat, I was concerned to find the soft brown fungus marching on to the throat; soon after as general a clearance as I could make, at a considerable loss of blood, the patient commenced the use of the pilul. arsenici three times a-day. A fortnight after, (May 15,) there was an evident amendment both in the general appearance of the mouth and in the tumour; his appetite increased, his bowels were regular, evacuations healthy, and his spirits were animated by his improved feelings. He continued, from choice and necessity, to work hard. June the 2d.—The gums, palate, and throat, looked healthy, had lost the brown appearance, were firm all round the chasm where the bone had been removed, and which had been so repeatedly fungous; but these promising evidences of acquired security were soon to experience the bitterness of disappointment. On continuing the medicine, the patient began to complain of return of his cough, of general uneasiness, of odd-nights, as he termed them, of short breath when pushing on his labour, and of increasing disinclination to work. The medicine was omitted. By the middle of the third week in June, a small fungous tubercle arose from the old spot, about the size and having the appearance of a red currant. He was averse to its immediate removal, but consented to rub it daily with a piece of ammon. mur.; but upon it acquiring the size of a walnut, he asked for the scalpel once more; it was sufficiently firm to admit of removal by one stroke. On pressing it between the fingers, it felt like brain. By persuasion of a friend, he one day took some gin and bitters to raise his spirits; a violent purging followed, with increased cough, and difficult expectoration, which compelled him to give up working. June 30.—The fungus had again resumed its original situation, and began to involve the upper lip; the man looked ill; his face sharp, his countenance sallow, as prior to the use of the pilul. arsen. I once more removed the fungus, and separated the new connection between the lip and gum, but owing to the flow of blood, was compelled to desist, against the wish of my patient. Feeling now too feeble to labour, he suddenly adopted the resolution of retiring to his native place, as he said, "to die." I saw him no more. The termination of this melancholy history is brief, taken from the oral statement of an affectionate wife. He lived to the first week of September. He once, soon after his arrival in his parish, applied to a charlatan, who sold him a six shilling bottle of black stuff to rub on the fungus, which he told him it would remove in a week, and he would be his own man again. This liquid made no impression on the fungus, which soon became so large as to fill the mouth, one side of the throat, and render speech inarticulate, with extreme difficulty of swallowing. His thirst was intense and incessant; his emaciation rapid; bowels extremely irregular. The tumour already mentioned reached from the angle of the jaw to the top of the shoulder; three new ones arose, one large, midway between the shoulder blades, and one on each side the nose, all moveable and elastic; his restlessness was extreme; cold unctuous sweats were nearly constant At last, blood of a fine florid colour flowed from his mouth, followed by dark clots, threatening suffocation. He appeared to suffer more from not being able to quench his thirst, than from any other cause. He would seize a bowl of water, and make the most dreadful despairing efforts to swallow its contents, while the terrified, but pitying beholders, saw the liquid regurgitating at the sides of his mouth, after every futile effort. These struggles aggravated the bleeding, but happily were soon followed by convulsion. In the form of a more than usually severe one, he expired in all the writhing agonies attending strangulation.

Chester, November 5, 1818.

V.

Two Successful Operations for Strangulated Hernia, performed on the sixth day. By Thomas Barnes, M. D. Member of the Royal College of Surgeons, London, and of the Royal Medical Society of Edinburgh, &c.

Case I.—J. A. ætatis 29, has been affected with a small inguinal hernia of the right side, for some years past, which frequently made its appearance after any violent exertion, but was easily replaced; this he effected himself, by pressure, and the recumbent posture. It returned on the 4th February, and became strangulated, attended with considerable pain of the tumour, and frequent vomiting. A surgeon was sent for on the 5th, who tried the taxis ineffectually, and ordered the administration of purgatives, and frequent injections; the purgatives were immediately rejected, feculent matter was voided along with the injections two or three times, and afterwards, the injections alone. On his symptoms increasing, the surgeon visited him again on the 6th instant. He requested them to persevere in the use of injections and purgatives, to employ fomentations to the part, and that the patient should make frequent attempts to reduce it himself. On the 7th, the surgeon being from home, another was called in, who supposing the hernia had been reduced, bled him to the extent of about ℥xii. and ordered them to continue the injections. The sufferings of the patient continuing to increase, and his friends becoming dissatisfied, a third surgeon was sent for on the night of the 8th inst. who, on their describing his situation, conceived the case desperate, and requested a physician to be called in, to meet him in consultation. He sent him a few effervescing draughts, to be taken in the meantime, to abate his vomiting.

9th.—Pain of the abdomen abated suddenly last night, but the sickness and vomiting became more frequent, and are now constant; the effervescent draughts were always rejected; the matter vomited is bilious. Has great anxiety of countenance, and frequent hiccup; considerable tension of the abdomen, and complains of pain all over the abdomen, aggravated on pressure. Pulse 120, small; tongue clean; much thirst; skin cool; extremities cold. No cause is assigned to his complaint, but cold and wet We considered it a case of enteritis, and thought the causes sufficient. We heard nothing of a hernia.*

Fiat venaesectio ad ℥xiv. ex brachio.

Habeat statim enema domesticum cum sulph. magnes. ℥i.

We left our patient with the intention of calling again in half an hour. While visiting another patient in the neighbourhood, inquiries were made after Mr A. and it was hinted to us, that he had lately a hernia. On our return, we examined him particularly, and found a small flat hernia, not half the size of a chicken's egg, in the right groin. Moderate attempts were made to reduce it; but as the patient complained of acute pain, on the slightest pressure, we desisted; and as any delay necessary for the application of other remedies might prove dangerous, I proposed an immediate operation, as the only means of preserving his life; and that I considered as a very doubtful remedy. The surgeon was of the same opinion; and both the patient and his friends readily agreed to it. An incision was begun at the upper part of the tumour, and carried down to about two inches in length. On approaching the hernial sac, it appeared much discoloured, but was tense and elastic; there was a little effusion around it; and it was compressed or constricted in the middle, by a band of fibres of the fascia covering it. On passing my finger along the sac, towards the abdominal ring, I found it impossible to reach the ring, without enlarging the incision; which being effected, I carried a probe-pointed bistoury along my finger, and divided the stricture, which was produced by the abdominal ring, directly upwards; the patient immediately screamed aloud; was seized with a sudden convulsion followed with fainting. I made gentle pressure on the tumour, and it entered the abdomen; the sac remained, but was flaccid; an opening was made into it, but no fluid escaped, and the intestine had entirely receded. The patient gradually recovered from his fainting. Three stitches of the interrupted suture were put in the wound; it was then covered with adhesive straps and compress, and the patient replaced in bed.

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* This shews the necessity of carefully examining the state of the abdomen, in every case of enteritis. Practitioners, however, must endeavour to avoid an opposite error. The author of these cases has witnessed three surgeons of eminence mistake an encysted tumour of the groin, in a case of enteritis, for strangulated hernia. An operation was performed, but, as might be expected, without any benefit; the tumour was extirpated, but the symptoms of inflammation continued to increase, and the patient died the following morning.

One hour after the operation.—Neither the vomiting nor hiccup have returned; pulse 112, but small; feels rather better.

> Sumat infusi sennæ ℥i. omni bihorio usque dum alvus solvatur. *Vespere* injiciatur enema catharticum ut supra præscriptum.

10th.—Was very restless in the beginning of the night; had frequent vomitings, and complained of much thirst, which abated towards morning; has had no vomiting since four o'clock, and has slept upwards of an hour very comfortably. Pulse 100, tolerably firm, but easily compressible; tongue clean; considerable borborygmus; not much tension of the abdomen, and can suffer considerable pressure without complaining; bowels not yet opened. The infusion of senna was obliged to be discontinued on account of the severe sickness it occasioned, and since four in the morning the following pills have been substituted, which have been retained without occasioning the least uneasiness.

> ℞ Calomelanos.
> Micæ panis *aa* ℈ii. Aquæ Q. S. fiat massa in pilulas xx. dividenda, quarum sumat duas tertia quaque horâ.

Two cathartic injections were administered during the night; the latter was tinged with excrement.

> Continuentur pilulæ usquedum plene dejiciatur alvus.
> Rep. enema catharticum, ter indies si opus sit.

11th.—Considerable tension of the abdomen, and pain of the right hypochondrium on pressure; has had several copious alvine evacuations; pulse 108.

> F. V. S. ad ℥x. Admoveatur emp. vesicatorium abdomini. Intermittantur pilulæ et enemata.

12th.—Blood covered with a thick buff; pulse 140; has passed a restless night; still complains of pain of the abdomen.

> Rep. V. S. ad ℥viii.

13th.—Blood buffy; pain diminished; pulse 128; no alvine evacuation.

Rept. enema catharticum.
Fiat V. S. ad ℥x.

14th.—Blood buffy; considerable pain upon pressure; pulse 120.

Rept. V. S. ad ℥xiv.
Admov. hirudines iii. hypochondrio dextro.
℞ Sulph. magnesiæ ℥ii.
Infusi rosae lb. i. solve. Sumat ℥ii. pro re nata ad alvum laxandam.

16th.—Blood buffy; the leeches were repeated yesterday; one dose of the medicine operated; wound suppurating; below it a small slough of the integuments has taken place. From this time he continued to improve. On the 23d, the slough had separated, and the wound was looking extremely well; pulse 88; appetite good; bowels open without the aid of medicine, except occasionally a common injection; feels himself in every respect much better. His diet is yet confined chiefly to liquids; he ate a little chicken two days ago, which caused a little uneasiness, since which time he has abstained altogether from animal food, except broth and beef-tea. On the 8th of March he continued convalescent, and was able to sit up occasionally; the wound was healing kindly, and he was allowed a more generous diet. An abscess had formed in his left arm, near to which he had been bled, it had been opened, and was then nearly well. Two months after the operation his recovery was complete. For the success of this case I am much indebted to my friend Mr Joshua Rigg, to whom the after-treatment was in a great measure committed.

Case II.—Mrs C. ætatis 63. Has for several years past been occasionally affected with a hernia of the right groin, which returned on the 28th June, and was accompanied with considerable pain, and succeeded with sickness and vomiting. On July 1st hiccup occurred, and the other symptoms became more distressing. I was desired to visit her on the 3d, (sixth of the disease.) She has now great anxiety of countenance, and her sickness, vomiting, and hiccup are almost constant; complains of pain all over the abdomen, but particularly in the vicinity of the tumour, which is much increased on the slightest pressure; the abdomen is swollen and tense; the tumour somewhat larger than a chicken's egg, is tense and very painful to the touch; pulse 80, intermitting every twentieth pulsation, rather full but not hard; has had no alvine evacuation since the return of the complaint. She is of a delicate constitution, and has not a tooth in her head. A variety of purgatives, emollient injections, fomentations, tincture of opium, and the taxis, have been frequently employed; a tobacco clyster has also been administered, which produced violent sickness and syncope, but the tumour could not be reduced.

ʒxiv. of blood were immediately taken, and I made several attempts at reduction, but in vain; a gurgling noise was heard, but no reduction of the tumour took place. The pulse rose to 90, and the intermission occurred every ten pulsations. As the last resource an operation was proposed, and readily agreed to. After dividing the integuments, a thin fascia, and the sac, about ℥i. of red coloured serum escaped, and the contents of the tumour made their appearance, the greatest part of which was omentum. A small piece of intestine highly inflamed, of a dark red colour, was on the back part of the tumour; about a table spoonful of gelatinous matter was adhering to the omentum on the posterior and lower part of the tumour; strong adhesions had formed between the sac and omentum, and also between the omentum and intestine, which, when separated, the stricture at the inner part of the hernia, commonly called Gimbernat's ligament, was divided with a probe-pointed bistoury, but the hernia could not be replaced; there still remained some adhesions at the superior part, some of which I separated with the handle, others I divided with the edge of the scalpel; I still found reduction difficult, and made a small incision of the stricture at the superior part of the hernia, which was then easily reduced. Two ligatures, adhesive straps, and compress were employed, and the patient put to bed. The hiccup was constant, but ceased at the end of half an hour; pulse 100, intermission not so frequent; the abdomen was ordered to be fomented; ℥xii. of blood were afterwards taken. The patient was very sick, but had no vomiting after the operation, and felt much less pain.

About an hour after the operation had one costive stool; an emollient injection was ordered to be given. Had two alvine evacuations during the night, and slept about an hour.

July 4th.—Complains of rather more pain in the abdomen, increased upon pressure; ℥xii. of blood were taken; six leeches were applied to the abdomen, and afterwards a blister; took ʒi. of castor oil at twice, and had an emollient injection; three stools during the day, the last of a very hard consistence.

5th.—Pulse 120, small and soft; one costive stool after the injection; passed a good night; pain diminished.

Sumat 2 p. ol. ricini ℥ss.
℞ Sulph. Magnesiæ ℥ii.
Infusi rosæ lb. i. Solve.
Capiat ℥ii. pro re nata.
Habeat omni nocte enema domesticum.

7th —Pain, swelling, and tension of the abdomen considerably diminished; pulse 104; bowels open; occasionally a little sickness; wound looks well.

9th.—Feels better; bowels open; pulse 100; a discharge of serum took place from the wound during the night. The pain of the right side of the abdomen having increased three leeches were applied, and afterwards a blister; wound healed at the superior part, red and rather swelled; the ligatures were removed, an emollient poultice was applied, and the medicines continued. From this time no unpleasant symptom occurred until the 16th; she had regular stools and not much pain of the abdomen; pulse 90, or between 90 and 100; a little tenderness of the abdomen, and considerable general debility still continued On the 16th a total cessation of pain and tenderness took place, accompanied with some sinking of her countenance, and also of her strength. On the 18th, a discharge of feculent matter; about a small wine-glassful in quantity, appearing to be of the same nature as the contents of the bowels, took place through the wound, which continued until the 21st, during which time she had constant drowsiness; pulse 80, sometimes intermitting.

Habeat vini rubri ℥viii. indies.
℞ Infusi cinchonæ ℥viii.
Tinct. ejusdem ℥i. M. Sumat ℥iss. bis indies.

On the 21st the drowsiness left her; she had two stools through the natural passage, and took rather more nourishment; pulse 84; the feculent discharge through the opening, which had gradually diminished, now ceased altogether; the wound continued to heal kindly. On the 24th the wound was nearly completely closed; bowels open; pulse 80, but still complained of great debility.

Cont. medicamenta et vinum.

After this time nothing material occurred; her recovery was slow but gradual. Her residence being upwards of twenty miles from Carlisle, my friend Mr George Elliot chiefly conducted the after treatment.

Carlisle, 27th August 1818.

P. S. Since the two cases of strangulated hernia were forwarded to you, I have been informed by letter from Mr Elliot of Mrs C.'s death. It occurred on the 10th of October, fourteen weeks after the operation. Mr Elliot writes to me thus: "Until September she could walk with the help of a staff, and frequently told me, she would be as well as ever if she only had a little more strength." In the second week of September a tumour made its appearance directly under the old cicatrix, accompanied with colic pains; it soon ulcerated, and a copious discharge of feculent matter took place. The opening was as large as the barrel of a large goose quill, and the discharge from it was sometimes a tea-cupful in twenty-four hours. It was frequently accompanied with flatus, and evidently consisted of liquid feces similar to what were passed *per anum.* The discharge of feces through the opening continued until her death.

Although these cases have been successfully operated upon on the sixth day, they are by no means intended to convey, that the operation can be deferred with safety until that period; on the contrary, many die of the complaint in a much shorter time. But this may be learned from them,—that patients ought to be allowed the benefit of an operation, even in the most advanced stages of the disease, as they will sometimes recover at so late a period as the sixth day, when the most unfavourable symptoms are present.

VI.

Account of the Epidemic Fever which prevailed in Trinidad in 1817. By JOHN M'CABE, M.D. Surgeon R. Y. R.

In the beginning of August 1817, a fever of a most malignant character made its appearance in the Island of Trinidad. The town of Port of Spain, the capital and seat of government, was the first, and continued to be the chief theatre of its devastations. The governor of the colony, and many gentlemen who held official places under the government, were among the first that were attacked with the fever. The florid and plethoric were chiefly the subjects of its attacks, and particularly the newly arrived Europeans, but no length of residence in the island appeared to afford much security against the disease, as very few Europeans escaped its attacks, however long resident in the colony. The fever, however, appeared less aggravated in such as had been long in the country; the newly arrived European, whenever attacked, had but little chance of recovery. The average proportion of deaths in the town of Port of Spain I have heard estimated at twenty daily. This proportion is probably too great. It, however, appears, that, in the course of about four months from the first appearance of the fever, about eight hundred persons had fallen victims to it, in the town and environs of Port of Spain!

The general prevalence of the fever suggested the idea of contagion, and the dreadful mortality that attended it, gave a most melancholy impression of its malignant nature, and cast a general gloom over society. In town the duties of government were necessarily suspended, its efficient members being generally affected by this dreaded disease. The stores and places of business were shut up; trade appeared to cease; all considerations of future benefit gave way to anxiety for present security. Such as had acquaintances in the country abandoned the town. A stillness pervaded the whole, but it was the stillness of fear and alarm.

As the medical practitioners of the island, as well as the military medical officers, were divided in opinion respecting the contagious or non-contagious nature of the fever, it may not be uninteresting to notice such circumstances as appear to be connected with this view of the subject.

The fever on its first appearance was said to be confined to one particular street, and even to a particular part of that street, and gradually to have spread itself from thence to other parts of the town. In whatever house the disease appeared, it generally continued there till it had passed through all the members of the family, or all the persons inhabiting that house. In the military hospital at Orange Grove, in the suburbs of the town, all the European soldiers, that acted as orderlies or attendants on the sick, were seized with the fever. Many of the civilian medical practitioners in town were also attacked with it; and every European military medical officer in the colony, without exception, in the course of about two months from the first appearance of the fever, had suffered one or more attacks of the disease.

From these facts a strong presumption might be inferred in favour of the opinion that the disease was contagious; and it would appear that the point must be conceded in favour of that opinion, unless other causes can be assigned which may be considered capable of occasioning the facts above mentioned, independent of contagion.

The weather during the prevalence of the fever was different from what the oldest inhabitants of the island could remember ever to have witnessed there; the thermometer in the shade at Saint Joseph's often rising as high as eighty-nine, averaging about six or seven degrees higher than the usual temperature of the place; the thermometer at Port of Spain varying from ninety-six to ninety-eight in the shade, between the hours of one and three o'clock in the afternoon, about which time the greatest degree of heat is experienced. The temperature of Port of Spain exceeds that of Saint Joseph's generally about six or seven degrees, although the distance between the two places is not more than seven miles. Occasional showers of rain were succeeded by a scorching sun, a sultry and oppressive heat, and a total stagnation of the atmosphere.

The Island of Trinidad is a low marshy soil, particularly in the vicinity of Port of Spain and Saint Joseph's; the face of the country is covered with trees and brushwood, and in every part of it there is the greatest luxuriance of vegetation. The action of the sun's rays on decayed and decaying vegetation, together with the moisture supplied by occasional showers of rain, incessantly kept up a continual putrefaction of vegetable matter; whilst not even a breath of air communicated a motion to the atmosphere, which thus continued saturated with these unwholesome effluvia.

In addition to these causes of unhealthiness, which at this time applied generally to the island, there were some local causes which applied particularly to Port of Spain. An immense marsh or swamp extends from the town along the south coast of the island. In the usual course of the wind, from the eastward and northward, the town is not subject to the influence of this marsh, but during the period of the sickness in town, whenever the stillness of the atmosphere was interrupted by a breeze, the wind continually blew from the southward and westward, and sweeping over a marsh of about four hundred square miles, carried to the town its noxious exhalations.

The government of the colony about this time had been engaged in paving the streets of Port of Spain, in digging new beds for rivers about the town that had been recently choked up by torrents of rain, and also in making and repairing wharfs on that side of the town, which, by the change of direction of the wind, was now the windward side, and over which the south and south-west wind blew from the great marsh before mentioned. The town itself is built on an alluvial soil, consisting of vegetable and other matter, capable of undergoing a decomposition, and which, when turned up and exposed to the action of a scorching sun, and to the oxygen of the atmosphere, was necessarily thrown into a state of active putrefaction.

Let us now consider these causes of disease which existed at the time the fever commenced, and see how far they may be considered capable of occasioning the facts before stated. The fever, I understand, first appeared in a street near the wharf, and in the immediate course or current of the wind blowing from the marsh, and which must have arrived at this particular point impregnated with the miasmata arising from the marsh, and further loaded with the noxious exhalations occasioned by the exposure of an alluvial soil. This street, I understand, was crowded with Spaniards from the Spanish Main, in a deplorable state of misery and of wretchedness. On such subjects, and under such circumstances, the causes of fever were likely to operate with double effect. Here the fever commenced, and from hence it spread to almost every part of the town. Here the effluvia arising from the bodies of the sick, the dead, and the dying, congregated together in a state at which humanity shudders, was added to the other noxious gases, with which, as I have already shown, the atmosphere must have been impregnated. If we add to this, that in the course of the mortality occasioned by the fever, about twenty graves were daily opened, or new ones dug, in the burial ground in the immediate vicinity of Port of Spain, we will easily perceive that the atmosphere generally throughout the town must soon have become impregnated, or rather saturated, with every noxious gas or exhalation capable of generating the seeds of disease.

Were I required to give an opinion whether the fever in Trinidad was contagious or not, I should be greatly at a loss what opinion to give, although I had the fever myself at the time, and although I endeavoured to notice every circumstance connected with it, that I considered might be likely to throw any light on this debated question. That the causes of the fever existed in the island, I believe, and that it was not occasioned by contagion or infection imported from any other place. If by contagion is meant the communication of fever by contact, I do not think the fever at Trinidad to have been contagious; but if by contagion we are to understand the accumulation or concentration of causes capable of inducing the action of fever, then the fever at Trinidad might be considered contagious; for it appears to me, that when the atmosphere of a whole town is loaded with noxious effluvia, as has already been mentioned to have been the case in Port of Spain, that the putrid exhalations arising from human bodies in the apartments of the sick and the dying, superadded to such an atmosphere, will be likely to accelerate the febrile action, and, consequently, that, in the apartments of the sick, a person is more susceptible of the disease than elsewhere, as there the atmosphere must be more impure, and the causes of fever more concentrated.

With respect to the character of the fever I had no opportunity of making any personal observations on it in Port of Spain; but from the accounts of such as have had that opportunity, and from the general fatality that attended it, it appears to have been most malignant. At Saint Joseph's its approaches were remarked by an unaccountable uneasiness and restlessness, a sense of unhappiness, without pain, or sickness, or any assignable cause. This was succeeded by a wandering of intellect, and an impossibility of fixing the attention on any subject, even for a few moments. In a short time the pulse became quickened, with a flushing of the face, and throbbing of the temporal arteries, attended with rigors; a slight delirium probably ensued, with nausea, thirst, and excitement, and irritability of the system. In about twenty-four hours, these symptoms were succeeded by great prostration of strength; the pulse sunk below the natural standard, and became feeble, even where bleeding had not been resorted to; from this period debility and faintness were the prominent features. In some cases an effusion took place, tinging the skin and eyes of a deep yellow colour, but this symptom was by no means general; however, it usually attended the most aggravated cases, but could not be considered a fatal symptom. The patient usually remained sensible throughout. When the case terminated fatally, it usually happened on the third or fourth day, sometimes earlier. A suppression of urine sometimes occurred, and whenever it happened the case terminated fatally. In many cases a vomiting of black matter occurred a few hours before death. This symptom did not accompany all the fatal cases, but whenever it happened it might be considered a fatal symptom.

This fever was generally considered what has been called by writers " the genuine Yellow Fever of the West Indies," and in cases where a suffusion took place, the term " yellow fever" is very expressive of its appearance. But as this symptom did not always occur, and when it did was not always a fatal one, and as many cases occurred of the most aggravated description, when not the slightest tinge or suffusion appeared, the term " yellow fever" could not with strict propriety be applied to it. Having carefully considered its form and character, the symptoms and progress of it when suffusion took place, and where this symptom did not occur, I conceive that it differs only in degree from the common endemic fever of Trinidad, cases of which will always be met with there, even during what they consider their most healthy seasons, or, in other words, that it was merely an aggravated form of the common bilious remittent fever of the island, and arising from the same sources, accumulated and concentrated by the causes already mentioned.

The great mortality that attended the fever in the town of Port of Spain, occasioned, I believe, a deviation from the usual modes of treatment. The medical practitioners, finding their usual remedies baffled, and their patients sinking, before they themselves had any idea of their danger, thought it necessary to resort to more powerful remedies, to combat such a formidable disease. The public also, alarmed at the want of success of the medical practitioners, looked out for such as would apply new and desperate remedies; so that it became a field of competition and experiment. The opposite principles of depletion, and of stimulating, were adopted by different practitioners. The one party bled and purged, and kept the patient as low as possible, and continued to do so till the stomach had lost its tone, and the introduction of nourishment, to support the exhaustion of the system, became impossible. After the patient had lost one hundred or more ounces of blood, and taken two hundred or more grains of calomel, nourishment was allowed him; but then it was usually too late; the stomach rejected it. Such as adopted this mode of treatment, generally complained that their patients died after the fever had been removed.

The stimulating plan, on the other hand, has been carried to an extent, I suppose, unheard of. Three bottles of brandy have been given to a patient in less than twenty hours, and the same proportion continued for several days! This treatment, I understand, has been in several cases successful, when the fever had assumed a very unfavourable aspect. I have myself seen some, after their recovery, with whom this treatment had been adopted. Although this circumstance does not prove that such patients might not have been saved by remedies less violent, it shews us, that the system, while under the influence of fever, can bear a quantity of stimuli of which we had no idea, without its producing a fatal result.

The mortality occasioned by the fever among the troops, was by no means in the same proportion as among the inhabitants; although the increase of sick among the military was to a most melancholy extent. It is true that the European troops were stationed at St Joseph's, and at Fort George, each post about seven miles from town; and it is probable that the fevers that occurred at these stations were not of so malignant a character as those that happened at Port of Spain. At the commencement of the sickly season, the number of sick in the regimental hospital of the Royal York Rangers, at St

Joseph's, amounted to about fifty-four, as nearly as I can recollect; the total number present was four hundred and eighty; and in the course of about twenty days from that period, the number of sick had increased to about one hundred and twenty! These were mostly cases of fever. This increase of sickness at St Joseph's will shew that the fever must have been occasioned by causes that generally affected the island; although, as I have already remarked, some local causes in town, superadded to these general ones, may have occasioned there a more malignant and concentrated form of the disease.

This fever, as it appeared among the Royal York Rangers at St Joseph's, did not bear the evacuations of bloodletting and purging, so well as the fevers that had usually occurred in the regiment. At that station, whatever mode of treatment was adopted at the commencement of the disease, debility and faintness, with a sinking of the pulse, and a general relaxation of the vital energies, were the leading features, after about twenty-four or thirty hours from the first attack; and where powerful purgations and copious bleeding had been practised in the beginning, these symptoms appeared in a more aggravated form. The most successful method was found—after bleeding, when necessary, and well evacuating the bowels with calomel, combined with cathartic extract, and assisted by a solution of neutral salts,—to moderate the irritability and excitement of the system with opiates; to keep the bowels moderately open, by occasional purgatives; to give frequently some nourishment, soup, arrow-root mixed with Madeira wine; and by alternating nourishment and wine with purgatives and enemas, to endeavour to maintain a balance in the system, so as to prevent violent reaction on the one hand, and, on the other, to support the action of the stomach and intestines, by a succession of moderate stimuli. Whenever the patient began to sink, and the pulse to flag, which, as I before mentioned, usually happened in thirty hours or less from the first attack, blisters were applied to the stomach and shoulders; cataplasms of the capsicum annuum to the feet and legs; boluses of camphor and opium were given internally; the nourishment was given frequently, and the quantity of wine increased, unless it disagreed with the patient's stomach; in which case, a little brandy and water was substituted for it. The proportion of wine it was seldom necessary to increase to more than a pint in twenty-four hours; but the great object was, to introduce a little frequently into the stomach; as experience had shewn, that unless the action of that organ was kept up, by frequently introducing something not likely to disagree with it, its tone shortly became destroyed, its action ceased, and with it, its influence on the system. The powers of life appeared then gradually to cease, as if for want of something to give motion to the circulating fluids. Absorption and suffusion took place; black and fetid bile was ejected from the stomach; probably not by its own action, but by the inverted action of the intestines, whose irritability overcame the feeble resistance of the stomach; and the unfortunate sufferer became absolutely putrid a considerable time before dissolution took place.

In about two months, eighty cases of this fever were treated in the regimental hospital of the Royal York Rangers; out of that number, as well as I can recollect, five died. The others recovered. It is true that all these cases of fever were not equally severe. Among them might be seen every variety of shade or difference, from the common remittent fever of the climate, to the most aggravated and malignant form of what is called "West India, or Yellow Fever." There were many, however, very severe; but I conceive that the whole might be considered of the same description, differing from each other only in degree of mildness or of malignity.

Among those that fell victims to this fever among the military was Mr Safe, surgeon to the forces, and principal medical officer in the colony. He resided in Port of Spain, and had a considerable share of the practice which this melancholy season but too extensively afforded them. He had made considerable exertions in the discharge of his public and private duties, which no doubt contributed to the fatal tendency of his disease. He was a man of mild and amiable manners, and an example of the most rigid abstemiousness in his habits of living. He afforded a melancholy instance, that habits the most regular, and temperance the most resolute, although always praiseworthy, afford but a feeble protection against the aggravated form of West India fever!

To doubt the efficacy of mercury in the cure of the fever of the West Indies may appear extraordinary;—a remedy now so generally employed, and carried to such an extent as almost to supersede all others in this disease;—a remedy so much admired by some practitioners, that I have heard them assert that they had given to a patient 200 grains of calomel in about 48 hours! The following circumstances, however, together with an impartial attention to its effects, induce me to doubt whether mercury be entitled to the great praises lavished on it in the cure of this disease, and the great reliance placed in it to the almost total exclusion of other remedies.

That the action of mercury on the system does not supersede the action of fever, every medical officer must have seen who has had charge of an hospital in the West Indies. There may be seen frequently patients dying of fever while under the influence of mercury; the fatal termination of the disease will there be observed to take place during any stage of the effects of this medicine, from a slight soreness of the gums to the most saturated stage of ptyalism. I have seen persons using mercury for the cure of other diseases; and, while ptyalism was present, attacked with fever, and die of it. In such cases, it also appeared that the progress to putrefaction during the disease, and after death, was even more rapid than in ordinary cases. I have seen a medical gentleman, a great admirer of mercury, who, during the prevalence of a malignant fever, by way of securing himself against the disease, took three grains of calomel every day. This gentleman was attacked with the fever, and died. With so many instances of at least its want of success in the removal or prevention of fever, is it not reasonable to doubt its right to the unqualified praises bestowed on this remedy?

Let us not, however, deprive this medicine of the degree of merit to which it is entitled in the treatment of West India fever. There is no other medicine which can be given as a purgative that will produce so good an effect in so small a dose; and when the stomach is incapable of retaining any other purgative, a small quantity of calomel will be generally retained. Having seen the inefficacy of mercurial ointment, used as rapidly as possible, and successfully so far as producing ptyalism; and having also repeatedly seen calomel thrown in in large quantities, with the same view of inducing the action of mercury in the system, fail in removing the fever, and greatly increase the symptoms of faintness and exhaustion, I am much inclined to think, that mercury, used with the view of saturating the system in fever, has very little effect in curing the disease, and that its beneficial effects principally depend on its action as a purgative. Should we give calomel in fever, without having any confidence in its specific qualities, and merely with a view to its action as a purgative, it can never be necessary, with that view, to give such an enormous quantity as 200 grains,—a quantity which I suppose a European practitioner would think likely to injure even a strong constitution. With a view to its purgative effects, it will seldom be necessary to exceed ten grains for a dose. This quantity, combined with as much of the extract. colocynth. comp. and their effects assisted by a solution of neutral salts, will in general produce the desired effect. Such a dose may be given, if necessary, three or four times within the first 48 hours after the attack of fever; and, after that period, it was found at St Joseph's more necessary to support the patient than to exhaust him by more purging medicine. If calomel is given with this view, it will seldom be necessary, and probably seldom advisable, to give a patient more than forty grains in the course of the disease. I have seen such a degree of faintness and exhaustion succeed to even such moderate doses as I have mentioned, that the pulse became almost imperceptible, and all the other functions were equally enfeebled. It was only by giving the patient frequently a little toasted bread, dipped in Madeira wine, that the action of the stomach was restored, that the pulse was raised, and that some degree of energy was again communicated to all the vital functions. The patient, in this case, recovered; but it is probable, that, had a greater quantity of medicine been used, and a proportionately greater degree of exhaustion been produced, the vital energies would have been so far deprived of their renovating powers, that no stimuli, however powerful, could again have called them into action.

VII.

Case of Laryngitis. By A. Wood, Member of the Royal College of Surgeons, London.

There are few diseases which present nicer points of discrimination, or require a mode of treatment more active and immediate, than inflammatory affections of the organs of voice. The rapidity with which inflammation generally introduces a state of the parts which is incompatible with healthy breathing, either from excessive irritability, or from an increase or alteration in the secretions, renders it of the utmost moment that the complaint should be speedily ascertained, and if possible arrested. The following case appears remarkable, since it shews the very considerable length of time which active inflammation of a limited extent of the mucous membrane of the larynx and trachea may exist, without producing the constitutional disorder, and obstructed respiration, which are its common results.

December 13.—Elizabeth Bentley, aged 10, is supposed by her parents to be suffering from the effects of a slight cold caught two or three days since; but the affection is so trifling, that their attention would, most probably, never have been directed to it, but for a noise which the child makes in breathing. This noise is the loudest during sleep, and at the time of inspiration; and it has the peculiar croaking sound which is characteristic of croup. Her voice is hoarse, and she seems as if speaking through a fluid. She describes the pain to be a pricking and dryness of her throat. On being requested to point out its seat, she places her fingers on the thyroid cartilage; and pressure on this part, particularly by placing a finger deep on each side, gives her great pain. The fauces have a perfectly natural appearance. She has a short cough, without expectoration; and it is evidently excited by the irritation of the larynx alone. She does not complain of pain in her chest or head. She has no thirst. Her tongue is clean, and her bowels are regular. Leeches were directed to be applied to the affected part; and afterwards, a blister; and she was ordered to take a dose of calomel and jalap. Till the 17th she seemed to recover. Her breathing had become more free, and attended with less noise; and her voice was equally improved. On the 18th, apparently from exposure to cold, her symptoms had become somewhat more severe. Leeches were again applied, and 3 grains of calomel were prescribed, to be taken every four hours, with the intention of producing salivation. 19th.—She was so much better after the bleeding, that her mother suspended the exhibition of the calomel, after she had taken four doses, under the idea that it was unnecessary. 20th and 21st.—Her breathing continued free and comfortable, and the pain, on pressure, was trifling; but her voice had gradually become more feeble, and she seldom spoke higher than a whisper. In consequence of a confined state of the bowels, a dose of opening medicine was given on each of these days. 22d.—There was little alteration. Her pulse was, as usual, about 85; and she had no constitutional disorder that could lead to the supposition of the existence of local irritation. As much had lately been said of the efficacy of balsam of capaiba in inflammation of mucous surfaces, I was desirous of trying it; and after some persuasion, I prevailed on the child's mother to give it as much as its stomach would bear, for two days. It produced no effect, and was given up; and after this time, the parents declined giving any more medicine; so much advantage having always been derived from bleeding and blistering, that they believed the complaint would at all times be under the control of these remedies. 26th.—There was no material change in the symptoms; and she was again bled. 27th.—She was blistered. 28th.—As had been invariably the case after the use of blisters, she breathed better, and had less cough. Her voice, however, had become exceedingly weak, and it gave her considerable pain to speak. She now and then was able to hawk up a little viscid phlegm. 29th.—I could detect no variation since the report of yesterday. 30th.—Her countenance was rather bloated. She had a dull headache. She was sleepy, and had other symptoms of congestion about the head. She spoke only in a whisper; or if she attempted to make her voice sonorous, she complained of the effort it required, and the pain she suffered in the larynx. The noise, which had been hitherto very loud during inspiration, and when she coughed, was now nearly inaudible. She had slight fever; and though she had been running about the house the whole day, she was more fatigued with it than she had ever been before. It was in vain that the mother was urged to resume the calomel powders. 31st.—This morning, about three o'clock, she suddenly awoke with a sense of suffocation in her throat, which continued nearly an hour. She was relieved by coughing up a quantity of phlegm; and she afterwards slept several hours very composedly. The bloatedness of the face has increased. Her pulse, which had never before been more than 90, was now 110; and her respirations were 50 in a minute. The irritation in the larynx is increasing, and gives her much distress. A blister was applied; and I left particular injunctions, that should any more paroxysms of difficult breathing come on, I should be immediately sent for; since it was now evident, that unless an opening was made into the trachea, she might perish from obstructed respiration. On calling in the evening, the mother smiled, and observed, there would be no occasion to send for me. My patient was eating with an appetite, and without much difficulty, the dry crust of an apple tart. The fauces, yesterday and to-day, had presented, for the first time, a scarcely perceptible blush of inflammation. The morning after, I was disappointed, in hearing that the girl was dead. About four o'clock in the morning, a fit of difficult breathing awoke her. She struggled for half an hour, became quiet, and after breathing for ten minutes, according to the mother's account, with perfect ease, she expired.

The examination was made in presence of my friend and partner, Dr Dunlop, under whose observation the case had been from its commencement. The larynx and the trachea, as far down as the upper bone of the sternum, were removed. On making the division of the trachea, about a tea-spoonful of transparent mucus escaped, similar to what had been coughed up during life. They were now divided on their posterior side, through their whole length, beginning at the cut extremity of the trachea, and terminating at the top of the larynx; and on gently separating the divided edges, it was found, that the blade of the scissors which had traversed the interior surface, had passed between the mucous lining of the part, and a perfect membrane of coagulable lymph. This membrane was of sufficient firmness to allow of the separation of the cut edges of cartilage, to at least half an inch, without suffering in its texture, readily parting from its secreting surface. This part of the adventitious lining was now divided, and it was found to be continued completely round the tube, forming a tolerably perfect cylinder, of different degrees of thickness, with a cavity in one part so small that a probe could barely be introduced without violence. The cavity was plugged up with mucus, with flakes of lymph mixed with it. I was not able to detect the least appearance of puriform matter. The deposition of lymph commenced at the root of the epiglottis in a jagged form, and, extending downwards, covered the chordæ vocales, sacculi laryngis, and nearly the whole of the internal surface of the larynx, till it arrived at the lower border of the cricoid cartilage, where it abruptly terminated in a well-defined edge, which was loose, and floating in mucus. On detaching the lymph, the mucous membrane beneath was found entire, and of a bright red colour, from inflammation. The marks of inflammation extended about a quarter of an inch into the trachea, and the whole of the under surface of the epiglottis, with the parts in the neighbourhood unoccupied by lymph, shewed marks of high increased action. Near the tip of the epiglottis there was a dark coloured spot, of about two lines in diameter, but not penetrating deeper than its mucous covering. We had not sufficient time allowed us to examine the chest.

That the inflammation in this case was of an active character there can be little doubt. The dissection exhibits phenomena which nothing but active inflammation could produce; for it appears an established law in the pathology of mucous membranes, that acute inflammation shall be productive of coagulable lymph, while pus, or a mere increase of the natural secretion, results from inflammation of a more indolent description.* It is not easy to account satisfactorily for the slow progress of the disease, except we admit that the regular manner in which the lymph was deposited opposed less obstruction to respiration than would, in all probability, have resulted from that thickening of the membrane, and œdematous effusion beneath it, which are the usual consequences of inflammation of a chronic kind. Moreover, I apprehend the secretion of mucus would be much more plentiful in cases of the latter kind; and the perpetual exertion to clear the trachea and larynx of this matter must tend in no inconsiderable degree to exhaust the patient. I am aware, that, in young children, this disease (or one so similar to it that I make no distinction) suddenly runs its course; but I believe this arises from the greater rapidity with which coagulable lymph is thrown out in them, and perhaps in some measure from the little power of expectoration which they possess.

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* "If this inflammation, which produced suppuration on those surfaces, (mucous surfaces,) becomes more violent, we find that it moves from the suppurative to the adhesive, and throws out coagulable lymph." Hunter on the Blood, &c.

Since the publication of the cases by Drs Baillie and Farre,* the nature of this disease, and its appropriate mode of treatment, have been very clearly understood. If the speculations of some modern pathologists† be well founded respecting the influence of a mercurial action over diseases of an inflammatory cast, the practice of an eminent northern physician, of exhibiting mercury in large doses, is one of the highest promise, and will possess the recommendation of being supported by theory as well as by experience. Setting aside this view of the subject, it is likely, from the vicinage and connections of the parts, that a copious secretion from the salivary glands and mucous lining of the mouth, will operate very directly and advantageously on inflammation of the trachea and larynx.

It becomes an interesting consideration, at the close of this paper, to inquire what would have been the probable result had tracheotomy been performed.‡ I have stated that the lower border of the effused lymph was "loose and floating." A process of separation was also going on at its upper part, and this process seemed only to require a sufficient length of time to have effected its complete detachment. There is every reason to believe, that, when this had taken place, the surface beneath would have been left without ulceration, and with not more inflammation than would readily have subsided on the removal of so direct a source of irritation. The morbid action going on in the epiglottis, and in the commencement of the trachea, was in all likelihood kept up by continuous sympathy with the part more actively inflamed; and I attribute the separation to the secretion of mucus, arising from a diminution of that violent inflammation which had produced coagulable lymph.

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* Medico-Chirurgical Transactions.

† Dr Hamilton, Dr Armstrong, Mr Johnson, &c.

‡ There is a very valuable paper, by Mr Lawrence, in the sixth volume of the Medico-Chirurgical Transactions, in which the question of bronchotomy is considered with much candour and ability. From the whole of the evidence, which Mr L. has collected with great diligence, he is strongly inclined to recommend it; and in a case quoted from the Philosophical Transactions, and in one of the author's own cases, the lives of the patients were manifestly saved by the operation.

In the same volume Mr Chevalier relates a case where the operation was required, and successfully done after a membranous substance had been coughed up. It seemed that the good effects arising from it were attributable to its giving vent to about "an ounce, or an ounce and a half, of a reddish-brown and frothy mucus."

Rochdale, Feb. 21, 1819.

VIII.

On a peculiar Species of Gangrenous Ulcer, which affects the Face in Children. By MARSHALL HALL, M. D. F. R. S. E. &c.

IT would appear that the skin and subjacent textures in children are particularly liable to assume a disposition to sloughing and gangrene. I have witnessed this occurrence in cases of intertrigo affecting the bend of the thigh and the fossa of the nates, in excoriation behind the ears, in the inflammation which sometimes supervenes on inoculation, in severe cases of aphthæ, and as an effect of the application of a blister. Sloughing and gangrene are also usual in the erysipelas infantile, and children appear to be particularly exposed to that species of inflammation and of gangrene termed frost-bite.

Children are also liable, in the circumstances about to be described, to a particular affection of the face, which begins with pain, hardness, swelling, and slight erythematous redness, and terminates in the formation of a spreading eschar and ulcer. In one case, the patient did not survive the extreme irritation of the system in general, which attends the commencement of this affection; in four other cases, life was prolonged until a considerable portion of the soft parts of the face and mouth was destroyed by the mortification, and the latter patients died from exhaustion; in a sixth, the patient survived the affection altogether, after experiencing an extensive sphacelation of each cheek, of a part of the tongue, and of the contiguous gums, and even of a portion of the jaw-bone.

In all the cases which have come to my knowledge, this affection had been *preceded* by fever, acute disorder of the digestive organs, typhus, inflammation of the lungs, variola, rubeola, or scarlatina. This affection would, therefore, appear to be in some measure the consequence of the exhaustion, debility, or irritation induced by previous disease. It might be proposed, indeed, as an interesting question, whether it may be an effect of calomel, so usually prescribed, in the present day, in almost all diseases of children. I am disposed, however, to reply to this question in the negative. For, in one of the instances of this affection about to be detailed, it appeared to be distinctly ascertained that no calomel had been administered ; and the effects of calomel on the mouth, even when so great as to induce sloughing and prove fatal, are manifestly different from those of the disease under consideration.

This affection has appeared to occur principally, and indeed almost exclusively, in *infancy*, in the *winter* season, and in *female* children, as may be collected from a reference to the cases subjoined.

In this disease, frequently when the little patient has appeared to be *convalescent* from the previous indisposition, some part of the face has become affected with pain, induration, swelling, and erythema, and the child has become cross, irritable, feverish, and restless. At no distant period, usually on the succeeding day, a dark-purple or livid spot has appeared, which has soon assumed a dark-brown colour, losing its purple hue, and at the same time its vitality. When the patient survives, the sphacelated part enlarges, and becomes black, separated, loose, and extremely fetid ; the living part retains an erythematous redness, bordered by a ring of a livid hue. The internal mouth is soon involved in the affection ; the sphacelus penetrating into this cavity, the teeth become loose, and eventually fall out, and the breath is shockingly offensive. The child, from being restless, gradually becomes more patient and tranquil ; it seems frequently conscious of the disgusting appearance of the affection, and dislikes to be noticed ; but there is often, eventually, dosing or coma. In the later stages, there is not much heat of skin, but the pulse is frequent.

This affection appears to be by no means very unfrequent, as, in conversations with different medical friends, I have found that each could recall to mind similar cases as having been cursorily observed by them in the usual hurried course of practice. It is noticed by Mr Dease, Dr Underwood, and Mr Burns, under the denomination of *erosion of the cheek.* Huxham, too, observes, in his report for July 1745, " I have more than once, during this month, witnessed a mortification of the fauces and mouth; and, besides, a caries of the cheeks and os vomeris, which occasioned a very painful kind of death, and that, too, after the measles." And, in the report for September, he adds, "Sometimes there were very terrible ulcers of the mouth, which occasioned a caries of the maxillæ." Dr Willan remarks, in relation to scarlatina, "In one infant, about the eleventh day, a considerable erysipelatous swelling affected the left cheek, and, within three days, produced a deep gangrenous eschar. The wrists, at the same time, became rigid and contracted, and the patient died on the sixteenth day of the disease. Another child, eight years old, was affected, on the seventeenth day, with an erysipelatous swelling under the left eye, and died on the following day. It would appear from authors, that there has often occurred superficial gangrene in other parts of the body, as about the nates and genitals, and likewise on the tongue, gums, and insides of the cheeks, sometimes attended with caries of the jawbones."

Dr Parr observes, under the head of *Nome*, that the term signifies "ulcers that sometimes attack the cheek or vulva of young girls. These appear in the form of red and somewhat livid spots; are not attended with pyrexia, pain, or tumour; and in a few days become gangrenous. These ulcers are usually considered as phagedænic." Mr Burns's account is still more distinct. * These notices of the nome may serve, perhaps, to connect the term with the disease described in this paper; or, being quoted here, they may lead to a more correct and accurate acceptation of this word. And, notwithstanding the accounts of this disease to which I have directed the reader's attention, I have thought that a detail of some cases of the affection might still be valuable, by diffusing a knowledge of its character at that early period at which alone a cure appears either probable or desirable; and that it might present a *counterpart* to a paper by Mr Kinder Wood † on a subject somewhat analogous, and, if the associations made by Dr Parr, and still more pointedly by Mr Burns, be correct, perhaps somewhat *connected* with the affection which forms the subject of this communication.

This affection must be principally distinguished from common parulis, erysipelas, canker, from the effect of mercury on the mouth, and from syphilis.

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* Principles of Midwifery, ed. 3. p. 499.

† Medico-Chirurgical Transactions, Vol. VII. p. 84.

I shall now subjoin the cases of this affection to which allusion was made in the commencement of this paper.

Case I.—November 1, 1817.—Mr Storer's little girl, aged 3 years. This infant became affected with rubeola eight days ago. The rash was very distinct on the fifth day, and has since that day been gradually declining. Yesterday she first complained of the under lip, which was observed to become affected, together with the left cheek, with a degree of hardness and tumour. This morning, besides the hardness and swelling, two minute livid or purplish spots were observed on the left part of the under lip, at its inner edge, where it is not supplied with cuticle, and so as to be partly within and partly without the cavity of the mouth. During this day the two spots have increased in magnitude, and have coalesced. The lip is affected with more hardness than swelling. This is distinctly felt on applying the finger. The hardness extends from the left to the right part of the under lip. The left cheek is also tumefied, and the left eye almost closed. The tongue is moist. The breath not yet fetid The teeth not loose. The bowels open. 2d.—The livid spot is somewhat more extensive, but the skin is unbroken, and it might be compared to the appearance in ecchymosis or purpura, affecting this part. But the colour is a bluer livid, and there is a considerable hardness, and some tumour of the whole lip towards the right side and towards the chin. The child is constantly applying its fingers to the part affected. The left side of the face is more swollen than before, and the left eye quite closed. The tongue is moist, the breath untainted, and the teeth still fast. The inside of the lips and the gums are unaffected. A third spot, besides the two which have united, was observed last night about the middle of the lip, and is rather larger this morning. The skin is cool. The pulse is frequent. Leeches, a cold lotion, and a purgative and saline medicine, were prescribed. 3d.—The spot is still somewhat increased in size. The middle part is now black; the edges of a purple livid colour. The hardness and swelling are also somewhat augmented; and the cheek has a glistening and erythematous appearance, without much redness. The left eye is quite closed. The right side of the face is slightly swollen. The breath is not fetid. The pulse is 192, and sharp. The bowels loose. There are great restlessness and uneasiness, but little external heat. The child is constantly picking the part affected with its fingers. 4th.—Little change since yesterday. 5th.—There is now a degree of fetor from the breath. The cheek and lip are still very hard, but the swelling is less considerable, and paler. The respiration is rather laborious. The pulse is softer. There is an appearance of sinking in the countenance and general aspect. The infant died a few hours after this report.

This is the only case under my observation in which death took place before the sphacelus had spread so as to destroy a great part of the soft parts of the cheek and internal mouth, and in which the patient appeared to die from the violence of the first symptoms.

Case II.—October 12, 1817.—Mrs Stone's little girl, aged 2 years and 2 months. This little girl was attacked with rubeola four weeks ago. A fortnight ago, when apparently only labouring under the consequent debility, she became affected with hardness, swelling, and palish redness of the upper lip and of the cheeks, especially the left. In a short time the gum in front of the upper jaw was observed to become livid, and the teeth became loose, and soon dropped from their sockets. About the sixth day of this affection a black spot appeared immediately under the left nostril; this spot gradually increased in magnitude; became separated and loose; emitted a most offensive odour, and was surrounded by an erythematous redness of the living parts. The disease extended until the lip, part of the alæ nasi, nose, and cheek, the front teeth, and part of the front of the upper jaw, were involved in sphacelus. The separated portion of flesh at length fell in, and exposed the internal mouth. Before death, too, the front of the under jaw and the under lip became affected. The under lip presented the same appearance of a circumscribed black spot, just below the lobe; this spot was surrounded by slight redness; it soon became a distinct, circumscribed, black, loose slough. The ulceration extended; the teeth fell out; and the gums participated in the affection. The fetor exhaled was excessive. Life was prolonged about fourteen days from the commencement of the affection, when the infant seemed to expire from exhaustion. During the course of the complaint it was generally restless, thirsty, and feverish.

Case III.—January, 1816.—Mr Smith's little girl, aged 4 years. She has had four attacks of inflammation of the lungs at different intervals, for which she was bled, &c. When apparently recovering from the last attack, the complexion was observed to become darker than usual; this appearance ceased, however, the following day. But a degree of swelling was remarked on the left side of the face; and she complained of toothache, and of pain of the head. On the first or second succeeding day, a black spot was observed on the side of the left ala nasi, and one of the teeth had fallen out and was found in bed. The breath was extremely fetid. The spot was surrounded by an erythematous redness and a degree of hardness. The little girl required to be rocked constantly. There was apparently little feverishness. The sphacelated spot increased daily. The teeth became loose and fell out. The breath continued excessively fetid. About the tenth day from the commencement of the affection the little patient expired.

Case IV.—January 4, 1817.—Mr Benson's little girl, aged 3½ years. She had been affected with fever about fourteen days, and was apparently convalescent, when a degree of swelling was perceived affecting the left side of the face, the left part of the lips, and closing the left eye. This swelling presented a red glistening appearance. About the same period three spots were observed, one on the gum of the under jaw, and the other two on the left cheek. Those on the cheek soon became dark-coloured. The child was extremely cross and restless, and constantly picking the part with its fingers. At first the progress of the affection was slow; but the spots gradually spread; a slough separated from the living part. The cheek was at length perforated, and the internal mouth exposed. The two sphacelated parts united. The dead part was retained in its place by means of a bandage; the teeth fell out. The breath and the exhalation from the ulcer were extremely offensive. The child lingered about fourteen days, and sunk gradually. There was little affection of the appetite or bowels, and the power of swallowing was retained to the last. She seemed sensible of the state of the face, and could not bear to be examined.

The following case is communicated to me by a friend.

Case V.—October 31, 1815.—Matilda Ellis, aged 5 years. She has been ill of worm-fever for some time; since which, a black spot has appeared on the cheek, and, having increased, a large hole is formed through the cheek. The mortification spreads rapidly, and will soon reach the mouth. November 3d and 5th.—The disease has spread still more. There is a secretion of pus at the part nearest the mouth. 8th.—There is a very extensive mortification of the left cheek; the surrounding integuments being erythematous and swollen. The mortification now occupies the fourth part of each lip on the left side, extending upwards within an inch of the orbit, backwards to the anterior edge of the masseter muscle, having the integuments over the zygoma untouched, and downwards to the basis of the lower jaw. The whole substance of the cheek has sloughed away to near the angle of the mouth, but elsewhere the inner lining of the mouth is still preserved. 11th.—The disease has spread a little towards the nose. 17th.—The disease still spreads. 18th.—The little patient died last night. About one-half of the nose is destroyed, and the mortification has extended to the left eye, and nearly to the left ear. The globe of the eye was not injured, but appears flatter than usual.

Case VI.—October, 1818.—Mary Barker, aged 12. In the second week of typhus fever, the gums and left side of the tongue were observed to have become black, and to be in a state of sloughing; the breath being extremely offensive, and the teeth loose. The face was much swollen. The inside of the *left* cheek, the left angle of the mouth, and a portion of the left side of the tongue, became, in a short time, affected with discoloration, succeeded by gangrenous sloughing and ulceration. About a week afterwards a gangrenous slough was observed to have formed in the middle of the *right* cheek, and the angle of the mouth of this side was observed to be eroded with ulceration. The two parts were torn into one by the patient during delirium. Much of the inside of the cheek, and a considerable portion of the angle of the mouth, sloughed. The gangrene was at length apparently arrested by poultices. Notwithstanding all this disease the patient has survived, and is now, May 4, 1819, in good general health. The *right* angle of the mouth has contracted and healed; but the cheek is much fallen in from the loss of substance. The *left* angle of the mouth displays a frightful chasm. Three teeth have fallen out, and a portion of the superior maxilla juts forward in a loose state, black, emitting an offensive odour, and covered by an exudation of pus, which issues from the alveola. The edges of the wound in the cheek are cicatrized.

Such was the deplorable aspect of this little girl in May. It was then proposed, when the removal of the diseased bone should have taken place, and the parts have become healthy, to join the opposite parts of the chasm in the left side of the cheek, by an operation similar to that for the hare-lip. But in the beginning of July a tendency to gangrene and sloughing again manifested itself, without any assignable cause. The soft parts at the opening at the left angle of the mouth became swollen and livid, the eschar and skin sloughed so as to enlarge the orifice considerably, and a portion of gum also separated. On this recurrence of the tendency to sloughing, the common linseed poultice was exchanged for a cataplasm of vinegar, chamomile, and linseed; this gave some pain, and appeared to arrest the sloughing; afterwards the parts healed, grew a little, and the orifice is at length become nearly as described in the last report. A portion of the alveolar processes, and side of the palate bone, and the second molar tooth, are carious, and moveable by the probe.

Nottingham, August 20, 1819.

IX.

Cases where Arsenic was accidentally swallowed by three Maidservants, and in which the poisonous action was succeeded by singular nervous affections. By ALEXANDER M'LEOD, Esq. Surgeon, North Uist.

ON Friday the 17th of July 1818, I was called to the Island of Vallay, North Uist, to visit three maidservants, who were said to have swallowed poison on the evening of the preceding day. On my arrival, Mrs Macdonald gave me the following account of the accident. About four o'clock of the evening of Thursday the 16th instant, Marion Mylis observed a brown substance, about the size of a walnut, on the lobby floor, which she took up, and supposed to be brown sugar. She instantly tasted it, and carried it where Kitty Macintyre and Anne Murray were spinning. She gave each of them a portion "of the sweet-meat she had found." Accordingly the three eat all she had picked up. They were soon after seized with violent retching, vomiting, and excruciating pain in the region of the stomach.

On their situation being made known to Mrs Macdonald, she concluded that what they had swallowed was of a poisonous quality, and therefore gave them large and repeated draughts of chamomile infusion, which was continued till they began to vomit blood; she then made them drink a quantity of warm milk, and gave 40 gt. of laudanum to each. They continued in great distress all the evening and during the night, complaining of heat and excruciating pain in their chests and throats, accompanied with a sense of stricture in the latter, with difficulty of swallowing, together with violent purging and discharge of blood by the anus. At bed-time, Mrs Macdonald gave them again another dose of laudanum. They got some respite during the night; and, on my arrival at nine o'clock A. M. Friday the 17th, I found them much easier, but complaining of pain in their chests, accompanied by heat and lancinating pain in their throats; and by profuse perspiration, and general debility. They had great thirst, and complained of pain in their teeth. I also observed purple petechiæ on their chests and necks.

I gave each of them a dose of castor oil, and ordered them to drink freely of warm milk or barley water. Towards evening, perceiving symptoms indicative of inflammation of the stomach in Marion Mylis, and her pulse being 104, and strong, I took eight ounces of blood from her arm, by which her pulse was reduced to 94, and she seemed to be much relieved.

I ordered them to take every hour a table spoonful of a mixture of the carb. potass. puriss. and sulph. sublim., which was continued so long as they had the power of swallowing.

Saturday the 18th, they all got out of bed, and found their complaints mitigated, but not removed. The physic given them the preceding evening operated during the night. I ordered each of them to take 12 gr. of the sulphuret of potash every two hours, in addition to the mixture of sulphur and potash prescribed the preceding day, and the physic to be repeated at night; also to continue taking the warm milk, which they drank in great quantities.

I was at this time called to see a patient at some distance. Between the hours of twelve and one in the afternoon, all their former symptoms increased in each of them, almost at the same time, viz. vomiting, purging, excessive pain in their stomachs, heat and pain in their throats, together with inflammation and swelling, internally and externally, about the root of their tongues. About three o'clock, Anne Murray lost the power of speech and swallowing, and shortly after her jaws became locked, accompanied by convulsive spasms over the rest of the body. About half past three o'clock, Marion Mylis became affected with similar symptoms. Kitty Macintyre, about the same time, lost the power of speech and swallowing, though her jaws did not appear to be locked. They continued in this state during the night, and no medicine could be administered.

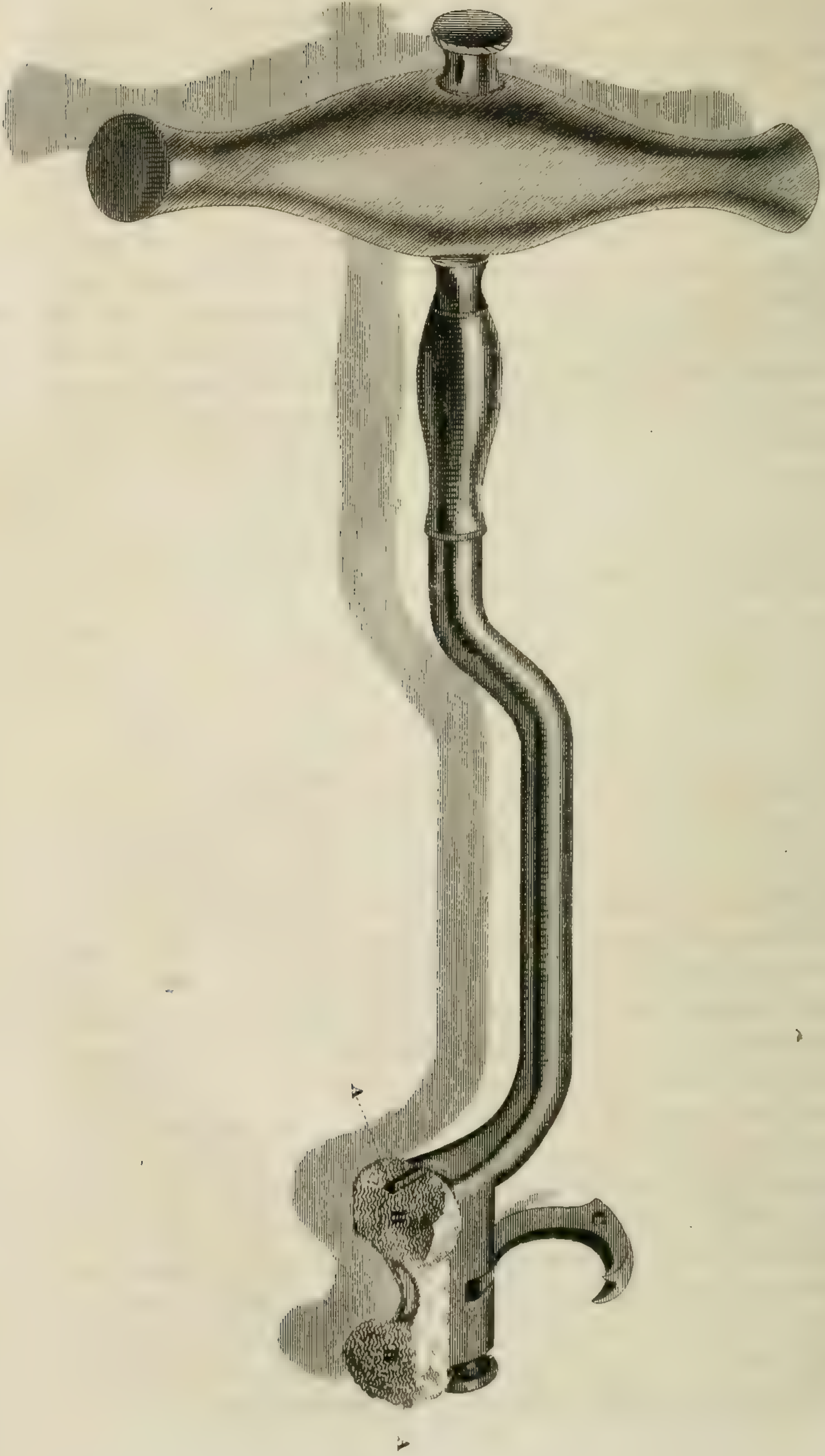
Owing to indisposition, I had it not in my power to see them before nine o'clock in the morning of Sunday the 19th, when Mr Donald Macqueen, surgeon, South Uist, accompanied me. We found Anne Murray in a state of apoplexy; breathing laborious, with convulsive startings all over her body; her jaws firmly locked; countenance pale and squalid; pulse 90, and feeble; and she was apparently insensible to external objects. We removed her to a cooler apartment, applied a large blister to the pit of her stomach, and another below the chin; took four ounces of blood from one of the jugular veins, and ordered her a strong injection. In about an hour thereafter, she could move her jaws, when the physic and sulphuret of potash were ordered as formerly. At twelve o'clock, I sprinkled her face with cold water, which had the effect of rousing her out of the comatose state she was in. She spoke to us at one o'clock, and complained of a violent headache, together with heat and pain in her throat. Her medicines were continued, and her feet were immersed for some time in warm water. She continued to recover the whole of this day.

At the time above mentioned, viz. nine o'clock in the morning of Sunday, we found Kitty Macintyre speechless, without the power of swallowing, and with her hand carried constantly towards her throat; her face was flushed; her pulse 104, and full; which determined us to take some blood from her arm. Before an ounce came away, she spoke, and said, Thank God I am relieved. We bled her to the extent of eight ounces; her medicines were repeated; and she continued tolerably easy during the evening, but occasionally complaining of headache.

On visiting Marion Mylis, at said time, we found her speechless, without the power of swallowing, her hand pointing to her throat, with occasional convulsive spasms over her body; her jaws were fixed, and she smiled frequently; her face was flushed; pulse 106, and strong. From seeing the good effects of bleeding in the other two cases, we drew off seven ounces from her arm. On the vein being opened, symptoms of hysteria came on, the spasms became stronger, her jaws more firmly fixed, and she smiled continually. These symptoms, however, wore off in a short time, and she recovered the power of speech and swallowing. The above medicines were given as formerly, and she passed a good night.

Finding them all in a fair way of recovery on the morning of Monday the 20th, I took my leave for that day. But, at nine o'clock in the evening, Anne Murray and Kitty Macintyre were nearly at the same time both seized with violent headache, succeeded soon after by high delirium, being insensible of their own situation, unmanageable, and not knowing any of their attendants. They listened to what was said to them, but always gave foolish answers to any questions that were asked. I found them in this state at four o'clock of the morning of Tuesday the 21st. But, as their pulses were very feeble, I did not venture to take any more blood. I gave to each of them a dose of salts, applied a large blister to the nape of their necks, ordered their feet to be immersed in warm water, gave them draughts of gruel, with some nitre. But finding, after a trial of five hours of these remedies, that they did not receive the least benefit,—at the same time judging that these secondary symptoms indicated inflammation of the brain,—I determined to try the effects of the affusion of cold water. I accordingly poured some of this several times over the head of the strongest of them, (Kitty Macintyre,) kept at the same time a towel dipped in the cold water to her head. The effects were almost instantaneous. Before the application of the water was continued for any length of time, a degree of shivering and gentle hiccough came on, when she recognized all in the room, became calm, and spoke perfectly sensibly. She said she had no pain, but felt a giddiness in her head, and general languor. Shortly after, she fell asleep, and wakened quite composed. I tried the same remedy, with some confidence, in the case of Anne Murray, and the result was exactly similar. Marion Mylis, who was in a separate house, on hearing that the other two were delirious, became so also, though not in so violent a degree. She continued in this state from the preceding day till the affusion of the cold water was used, when the same happy result followed as in the other

MR. HARDYS IMPROVED TOOTH-KEY.



Eng'd by W. H. Lizars. Edinburgh

Published by A. Constable & Co. Edinburgh 1819

two cases. This treatment was continued for some days, together with some doses of physic. They gradually recovered strength, without any return of their complaints, from this date; and are now (three months after the accident) in good health.

On the day the accident happened, Mrs Macdonald made a search, and found more of what had been swallowed near the place where the rest had been picked up by Marion Mylis. She kept it for my inspection. On looking at it, it appeared to be a mixture of brown sugar and hogs lard, with a white powder imperfectly mixed in it. I tried some of it on live coals, and was sensible of the garlic smell of arsenic from it. I afterwards tried an experiment between two pieces of copper, and found it gave them a whitish colour. The remaining part I kept to be analyzed by some person more able to do so than myself. I found out afterwards that the mixture was laid at some other place at a distance for killing rats, and got to Vallay by accident.

North Uist.

X.

Description of an Improved Toothkey. By Thomas Hardy, Surgeon and Dentist, Edinburgh. *(With an Engraving.)*

Some months ago I published a few observations upon extraction of the teeth, explanatory of an improvement upon the toothkey, which occurred to me about twelve months ago, the result of directing my attention constantly, in the course of my practice, to the action of the common toothkey, and the injuries occasioned by the single steel fulcrum pressing upon the alveolar processes immediately over the tooth to be extracted. I have used this improvement upon the instrument always since I first made trial of it, and am every day more pleased with the facility with which the firmest molares are extracted by it. I should wish much to endeavour to remove, through the medium of your widely circulated Journal, one or two objections which have been mentioned to me lately, as I have now more experience in the exact operation of the instrument. Before doing so, I shall notice the different instruments used in extraction, my reasons for being dissatisfied with the common toothkey, and explain the construction and action of the improvement I propose. The instruments which have been used in extraction of the teeth have assumed, at different times, many and various shapes. The claw of the toothkey now used in removing the molares, when well applied, acts certainly in the precise direction required. Many plans have been proposed for perpendicular extraction, but every one acquainted with the anatomy of the teeth, and with the mechanical powers, must admit that all attempts at this, wherever the fulcrum be placed, will require a greater force to unseat a tooth than that required in using the claw, which first gives it a lateral dislocation. Besides greater difficulty, there is infinitely more danger of extension of fracture of the alveolar processes, and of injury to the jaw or neighbouring teeth, in any form of perpendicular extraction. The most perfect toothkey now used is that with a round bolster, and the claw fixed on a spindle by which it is moved. This is useful in the extraction of the third molares or wisdom teeth. The intention of it, however, is for the position of the claw upon any tooth, without the introduction of the finger into the mouth. I should never wish to trust to this alone; for I consider fixing the instrument upon the proper part of the tooth to be a matter of the first importance, and the finger the best way of ascertaining whether or not it is in the best position. In the wisdom teeth this is often not so easily done, and then it is a useful addition. There is much nicety required in placing the fulcrum in the situation where it may act in the proper direction. The claw should be placed as far upon the neck of the tooth as possible, but the fulcrum may be too high or too low; the one attended with risk of breaking over the tooth, and the other of tearing away a considerable piece of the alveolar process. As the teeth vary so much in their situation and shape, no rule or plate of this can be well given. A little practice on the dead subject will much sooner direct the young operator. The other instruments in use are simple. For the removal of the anterior teeth, the forceps are employed. They are, however, generally made too sharp at the points, so that if the force exerted be considerable, the tooth is in danger of being cut over. They should take hold by rather blunt double points. For the extraction of loose stumps, the curved forceps are very necessary; but they seldom succeed where the roots are firm. In these cases I would recommend, after deep scarification, the small toothkey used for children, where it can possibly be employed. A small claw, with a slender point, fitted to the key, will be found very useful.

My great objections to the toothkey now in use are the injury done to the soft parts, and even to the jaw, in the removal of firm grinders, the consequences of which are often very unpleasant. The instrument often acts against itself, by the fulcrum pressing upon the alveolar process, immediately over a long root; the claw, thus exerting a great extractive power upon one part of the tooth, while the fulcrum is retaining it at another. This, I am confident, accounts for many cases of fracture at the neck of a tooth. The fulcrum, pressing upon a part relative to the tooth, must increase the pain of extracting greatly, in almost every case, on account of the preternatural sensibility at the time. The knowledge of all these evils attached to the common toothkey inclined me to attempt another construction of the fulcrum. After devoting much attention to it, in many repeated trials, I invented the form of the instrument seen in the plate, which I am confident will be found to remove, in a great degree, all these objections. It has the fulcrum lengthened to nearly an inch, the steel part of which is made of the same depth as that of the common toothkey, except at the centre, where it is not so deep; the thickness is less than an eighth of an inch. On each side of this, a double-headed bolster of fine firm cork is fixed, by means of silk thread passed through it, and holes made in the steel, for more easy renewal when required. The claw is exactly in the middle. The effect of this plan of the fulcrum is easily seen. Dividing, it throws the force on two points; and these points being made of yielding substance, the pressure is thereby so much lessened, that I have never hitherto seen any patient complain of injury to the gum or jaw. That part relative to the tooth to be removed is left free, so that it comes away with much less force in all cases. One great chance of breaking a tooth is done away, and the support given to the alveolar processes prevents fracture of them extending over other teeth.

I mean this instrument entirely for the removal of the grinders; they are, indeed, the only difficult and dangerous extractions. The anterior teeth are easily removed without danger, and the wisdom teeth require the position of the claw before the fulcrum, on account of the form of the jaw. Since the first publication of this improvement, the instrument has been tried with every success by several of my friends. Others have mentioned to me some objections which had been stated to them, but which I think very easily removed. I have heard of no objection being made to the principle of the instrument. The cork alone has been found fault with. I was told, that, in some instances, the cork gave way entirely under the pressure;

I think this impossible, where it is mounted properly. I have several times seen them mounted with cork in a way very different from what I wished. The cork is sometimes made considerably broader than the steel, so that, when the pressure is made, it must necessarily be twisted over the steel, and destroyed. Attention ought to be paid to procuring good cork, and placing it on the steel in the long direction of its structure. Another objection stated was, that the cork may be apt to imbibe moisture, thereby swell, and become too soft. In order to obviate this, it was proposed to substitute elastic gum, which, however, I have found not to answer so well. I have several times extracted with a toothkey mounted in this way, but have not yet sufficiently ascertained its qualities. From the very few seconds the instrument, covered with a bit of muslin, should be in the patient's mouth, and not being exposed even then to a great deal of moisture, I cannot conceive how the cork can acquire so much as to soften it in the least. Cork will be found to resist water entering its substance for an hour or two, even in water at a much higher temperature than the heat of the mouth. The instrument which I have in daily use proves these objections decidedly groundless. It has been used more than seventy times; the cork has never swelled, and is still perfectly in shape.

No additional power is given to the operator by this instrument, yet the manual force is much less than that required in using the other. This is certainly a proof of its being much easier for the patient. Its dividing the fulcrum greatly diminishes the pain and danger in this harsh operation. If the operator dislikes the application of cork, of which I have fully tried the good effects, great advantage will still be gained, although the bolster be made of any other substance, such as elastic gum, leather, or even iron.

AA. The fulcrum, nearly an inch long, not so deep at the centre as at the two ends.

BB. The bolsters, made of one piece of firm cork, neatly shaped, and both sides made exactly alike.

C. The claw, with a small notch.

Edinburgh, August 1819.

PART II.

CRITICAL ANALYSIS.

I.

Precis Elementaire de Physiologie. Par F. Magendie, Docteur en Medecine de la Faculté de Paris, Professeur d'Anatomie, de Physiologie, et de Semiotique, &c. 2 tomes 8vo. 1816, 1817.

The French physiological school cannot be said to have had any regular character before the appearance of Bichat; as the dreams of Bordeau and La Caze concerning the vital principle, and its negative, indescribable, qualities, are rather to be considered as metaphysical speculations than any thing strictly physiological. The whole amounted to this: that whatever appearances Bordeau and those of the school of Montpelier could not explain by our common ideas of the laws of matter and motion, they referred to the vital principle; exactly in the way that formerly the supporters of the peripatetic school referred them to *occult qualities.* A good deal of this way of talking and thinking (in our opinion a great deal too much) has appeared in this country since the days of John Hunter; but whether imported from Montpelier, or taken up from the many singular ideas of that ingenious man, does not appear. Such opinions only do harm, because they apply to every thing, yet explain nothing; and thus, by rendering persons unaccustomed to reflect, exceedingly dogmatical, without advancing their knowledge, cause them to adhere most pertinaciously to a principle, which, in their own view of it, is capable of illustrating every phenomenon of the living body.

The early genius, the industry, and modest, though practical mind of *Xavier Bichat* are known to every one. A thorough knowledge of the ancient languages enabled him to read with profit the many excellent works of anatomy and physiology to be found in Latin; a means often neglected by his countrymen, but by which his knowledge of physiology became much more extensive and solid than it could possibly have been otherwise.

Yet, with all these advantages and endowments, one is astonished at the extent of his views, at the novelty of his observations, and the specious, systematic train of his reasoning. The latter, however, is often more than ambiguous; and, in proof of the remark that physiology has rarely received any solid advantage from acute reasoning, we cannot give our readers a better instance than the theoretical inductions of Bichat, scarcely one of which are now taught or believed any where,* (except in Dublin,†) though scarce twenty years have elapsed since their first promulgation. They embrace, in particular, his views of *animal* and *organic* life; of the seat of the passions in the *animal* organs; of the sympathies and connections between the brain, heart, and lungs; of changes the bloodvessels of a viscus undergo from compression; of life, and of death. All these, with the exceptions above quoted, are now in general considered as curious but crude speculations. He conceived that man was possessed of two kinds of sensibility; *sensibility purely organic*, which takes place without his being conscious of it; and *sensibility of relation*, of which we are fully conscious, being the common sensibility whereby we are connected with the substances surrounding us. The first of these presides over *internal* or *organic* life; the second over *external* or *animal* life. *Organic life* consists in the compositions and decompositions of the animal in the processes of assimilation and excretion. *Animal life* receives impressions, and communicates them to the sensorium; a faculty peculiar to animals, denied to vegetables. Organic sensibility is only an inferior degree of sensibility of relation, and passes into it when rendered intense.

In this way we see this ingenious person divide the actions of which life is composed into two classes; and, had it suited his purpose, he might have divided them into twenty. He might have made a sensibility of *secretion*, a life of *secretion;* a sensibility of *nutrition*, a life of *nutrition;* and so on with all the other functions. For there is no better, nor so good a reason for considering these two lives, or rather *properties* of the functions, to be essentially different from each other, than there is for considering as various two functions that are known to all to be distinct. On this doctrine of a double life M. Bichat founded the division and scope of his "*Recherches sur la Vie et la Mort*," a work which would have been doubly valuable if not perplexed with this most unnatural opinion of a life and a sensation existing within us, without our being able to perceive it, and, indeed, almost independent of our existence. The other speculations which we have just hinted above, though less dazzling than this one of a latent life, are much better supported by their author, though the suite of experiments accommodated to each, and the mechanical induction tagged to the end of them, have made few converts among physiologists.

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* Magendie, Phys. I. 18, 185. II. 27, 28, 214, 309, 337.

† Vid. Blackwood's Magazine, No. XXII. 439.

But the strength of Xavier Bichat lay not in decisive experiments or logical inductions. His talent was for observation and comparison, for which latter operation his great memory and vivid impressions qualified him in an eminent degree. Accordingly, it is his work on membranes, and the fuller developement of his notions in the "*Anatomie Generale*," on which his reputation as an original genius must ever rest.

Such is likewise the opinion of M. Magendie; but that well informed writer does not seem to be aware of how large a portion of the work on membranes *appears* to have been pilfered from "Bohn, *De Continuationibus Membranorum*," a work published at Amsterdam so early as 1763, but which seems to have made little noise, as it has entirely escaped the notice of Haller. It is to be found in the second volume of Sandifort's Thesaurus; and few will be able to peruse it without a conviction that even the candid mind of Bichat had not been proof against the temptation to undue appropriation, which an obscure work of merit always presents to a professional writer. However this may be, the new and practical views which the division and distinctions of the different orders of membranes, and particularly of the serous and mucous, afforded, are highly interesting and important; and, when published, instantly caught the attention and admiration of the medical world. They have since, with various modifications, been adopted everywhere. These views of the different structures, and particularly of membranous structure, (whether derived from Bohn or Bichat,) have certainly been productive of considerable improvement in physic and surgery; but we doubt much whether they are to be considered as properly physiological, or if they can at all be identified with the present state of physiology in France, though their surgeons have not failed to profit by them. They are, in fact, anatomical remarks,* consisting of a more minute consideration of the structure and nutrition of the different tissues, and, above all, of their distinguishing characters; and are very justly included under the title of general anatomy by the author himself. It is this, indeed, that constitutes in a great part their value; for, different from most discoveries in medicine or general physics, they seldom require us to rest our belief on the individual assertion of the discoverer, the rigour of his inductions, or the fidelity of his experiments. On the contrary, we are astonished that we ourselves had not made the remark long before, and search in vain among the older anatomists and physiologists to detect an observation apparently so superficial. In the characters and distinctions of serous and mucous membranes, for example, we feel that we have noticed all that he says, or might have noticed it, long ago, but never thought of applying these insulated facts to constitute discriminating characters of two structures so frequently confounded. In short, we are the wise men of Gotham, who, though all present, ever fall short by one in the reckoning, till an intelligent passenger furnishes us with the hint, and leaves us astonished at our own simplicity. We have most of the facts, and might long since have had them all in our possession, but it was necessary that a Bichat should instruct us in an useful and practical application.

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* "La Base de ce plan est Anatomique." Pref. to An. Generale.

Some have imagined that the whole merit of Bichat, and of the French school which follows him, is confined to giving each fabric of the body a distinct name, and thereby directing our attention in a more particular manner to its structure and composition. This is not the case. Those divisions do not at all interfere with the advantages we have been delineating, and therefore constitute but a small share of their merit; yet it cannot be denied that they form the distinguishing feature, and that a preposterous attention to these little schedules of the parts and functions of the human body has been paid by the French writers on physiology.

For example, Dumas, whom we may consider as the latest physiologist of the Montpelier school, gives the following parts of the human body as *systems:*

1. Nervous, or sensitive.
2. Muscular, or moving.
3. Vascular, or calorific.
4. Visceral, or reparative.
5. Lymphatic, or collective.
6. Sexual, or reproductive.
7. Osseous, or fundamental.

But the number of Bichat's systems extends much beyond these of Dumas; and, on the whole, approach much nearer to the variety of nature. They are as follows:

1. Cellular.
2. Nervous.
3. Vascular, { red blood.
   black blood.
4. Capillary.
5. Exhalant.
6. Absorbent.
7. Osseous.
8. Medullary.
9. Cartilaginous.
10. Fibrous.
11. Fibro-cartilaginous.
12. Muscular.
13. Mucous.
14. Serous.
15. Synovial.
16. Glandular.
17. Dermoid.
18. Epidermoid.
19. Hairy.

It appears from Magendie that even this arrangement of Bichat is no longer the *ton* in France. The following is given by him as the least exceptionable. It is, however, a modification of the preceding, according to the ideas of Dupuytren, Richerand, and Magendie. *

1. Cellular.
2. Vascular,
   - Arterial.
   - Venous.
   - Lymphatic.
3. Nervous,
   - Cerebral.
   - Gangliac.
4. Osseous.
5. Fibrous,
   - Fibrous.
   - Fibro-cartilaginous.
   - Dermoid, or cuticular.
6. Muscular,
   - Voluntary.
   - Involuntary.
7. Erectile.
8. Mucous.
9. Serous.
10. Horny or epidermoid,
    - Hairy.
    - Epidermoid.
11. Parenchymatous,
    - Glandular.

Bichat, however, differs still farther from the arrangement of Magendie, as his theory of the twofold nature of life necessarily required. Thus he has a nervous system of animal life, and a nervous system of organic life; a muscular system of animal life, and a muscular system of organic life. Yet the two latter seem to be retained by Magendie and Richerand, under the title of voluntary and involuntary. These tables of systems may exhibit in one view the structures in which the body is divided by the physiologists of France, and, indeed, they constitute a striking feature of their science. But *cui bono?* In this country no *systems* have been published for many years, though at no period have the respective departments of physiological science been cultivated with more ardour and success. Hence such tabular views to us appear trivial and superfluous, though amongst our ingenious neighbours they seem as necessary to a book of physiology as the title and preface.

In selecting for examination the popular work of M. Magendie, entitled "*Precis Elementaire de Physiologie*," we had some hopes of having at least found a work to supply the general and confessed want of a good system of physiology. In this expectation our duty to the public compels us to acknowledge that we have been disappointed. The author's modest confession in the preface, of its being merely designed as a manual for students, ought, indeed, to have precluded extravagant anticipations, as it is hardly to be supposed that a book designed merely for that purpose should come up to the copious precision of a system. Nevertheless, as we have always believed that a great proportion of irrelevant matter is introduced into works of physiology, we did not conceive it impossible for a diligent writer, by avoiding these, to confine the science within the limits assumed by the author of this treatise. The particulars in which we have experienced disappointment will be found in the course of the analysis.

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* Richerand, in a late edition, gives a different arrangement!

His method is luminous and uninvolved; his language clear and perspicuous; so that no flashes of maudlin rhetoric, such as seem to run away with Dumas and Richerand, impede or disgust the reader. In this respect he is entitled to unqualified praise; and is, we believe, almost the first of his countrymen who have merited it on the present subject. We cannot say quite so much for the "*ardor enitendi*;" we suspect it has more than sufficiently manifested itself throughout the work, and not without a few traces of those other national failings so graphically described in the preface to the first volume of Haller's * Physiology.

M. Magendie's method is shortly this.—All the phenomena of life are reducible to *nutrition* and *vital action;* whose manifestations united, constitute general life. These he divides into functions of *relation*, functions of *nutrition*, functions of *generation;* which he again subdivides according to their connection with the primary division. Thus *functions* of relation are composed of sensations, understanding, voice, and motion. Functions of nutrition consist of six subordinate operations. 1. Digestion; 2. Absorption, and course of the chyle; 3. Course of the lymph; 4. Course of the venous blood; 5. Respiration; 6. Course of the arterial blood. This is the lamest part of M. Magendie's division, compelling him, as he confesses, (II. 1.) to leave out the secretions, which he does not consider as functions, but as actions of insulated organs. The functions of generation he does not subdivide. In treating of any individual function, Magendie pursues the following order, which we think tolerably judicious:

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* Urging the necessity of a careful collection of physiological facts into one system, he anticipates objections:—"Audio reclamantes librorum contemptores, qui nihil legunt, nisi noviter inventum; qui AUCTORES nunquam nominant, quin una refutent. Vulgo ità sentitur in gente ingeniosa et acri, quam à legendis libris saepe *linguarum* ignorantia absterret; quam ad naturam audiendam opportunitas incidendorum cadaverum invitat; quam denique ARDOR ENITENDI, praemiorumque, et academicorum titulorum cupido sollicitat. —Verum nimis acres hi viri erunt, si ideo nolent, nos verè per libros proficere, et ea forte ratio est, quae intercessit, quo minus longè passim, et in anatome et in physiologia progrederentur. Coram video exemplum, virum qui experimenta facit, et nonnunquam repetit, ingenio vero etiam in ea gente excellit, quae ingenio eminet. Verum non legit, ignorat rationes dudum in utrumque sensum propositas, non percipit, quantis, quam dudum enarratis, difficultatibus ea hypothesis prematur, quam confidenter proponit."

1. *General idea* of the function.
2. Circumstances which put the *organs* in action, or excitements.
3. Summary anatomical descriptions of these organs.
4. *Particular* consideration of the individual actions of the organs.
5. General review, shewing the utility of the function.
6. Relations of the function with those previously examined.
7. Modifications of the function.

It is evident, however, that, if our author had put his *third* division foremost, that the *first* and *fourth* might very properly have been thrown into one, and taken the *second* place, while the excitements would have come in very well immediately after them.

So much for the plan, which, nevertheless, is not inviolably preserved throughout the work; a circumstance for which we shall never dispute with the author, as we are conscious that it is "more honoured in the breach than in the observance," under those heads wherein he has ventured to deviate; * and only mention it from having observed an air of scholastic exactness assumed by our author and M. Richerand, as well as Bichat, for which we do not think their works the more valuable, but with which they ought certainly to be consistent.

Ever since the introduction of the Newtonian philosophy into France, which, as every one knows, was at a very late period of its history, mathematical reasoning has been the rage; and, as the philosophers of Europe seem to unite in according to our English *Bacon* the high honour of restoring, or rather creating, the sciences,—at any rate of teaching the world a right method of prosecuting them,—the French rave about *Bacon* too. Thus, admitting the primary proposition, which is at best doubtful, (however confident the author himself,) nothing scientific can be written, not even an ephemeral manual of the outlines of physiology for the use of students, but it must be composed *according to the method of induction.* "Of all cants," says a wit, "the cant of criticism is the worst;" but, setting aside a natural partiality for our brethren, we seriously do maintain, that the cant of philosophy is something still more detestable. Bacon's writings are like the Scriptures, which every one quotes, and no one reads. Their language is peculiar, and formed to express his own peculiar abstract notions, whether trivial or profound,—often highly metaphorical, insufferably quaint, or couched in the phrases of a logic which, it is to be hoped, is now forgotten for ever. His Latin is barbarous, his English less readable than his Latin; and, were it not for the luminous figures and aphoristic sentences which are every where scattered over his quartos, the most philosophic minds would feel them unconquerably repulsive. No wonder, then, if persons of a different turn should often dip into them without deriving any thing but error from their perusal. Of this a curious instance is afforded us in the preface of M. Magendie, though we wish it distinctly understood, that, although now reviewing his book, we do not mean to point out that author as singular in this respect. It is an error every day met with in French authors, in scarcely fewer of our own country, and is perpetually recurring in literary conversation. We allude to that part of his preface in which he proposes to conduct the *Precis Elementaire de Physiologie* "by the method of induction!"

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* Vol. I. 196. 242. Bacon, de Augm. Scient. Lib. VI. cap. II.

Through some misconception, partly, perhaps, from an inherent difficulty of the thing itself, M. Magendie, and many others, (even some writers on logic,) confound the *analytic method* with the *method of induction.* They are by no means identical. What in strict logic, and, of course, in mathematics, is called *analysis*, is that process by which we *assume* the proposition advanced as *true*, and reason from thence till we either arrive at known truths or a *reductio ad absurdum.* On the other hand, *synthesis* is when we put together facts *already known*, till, by their comparison, we arrive at the proposition to be demonstrated; a process which may often be easily performed by reversing the process of analysis. Whoever has solved a simple equation, or found the conditions impossible, has demonstrated a proposition in the analytic method; whoever has fully understood a proposition in plain geometry with its demonstration, has performed an example of synthesis. These two methods, perfectly known to the ancients, (as Euclid's data sufficiently shews,) existed in full perfection before either Galileo or "*l'illustre Chancelier*" were in embryo. Indeed, the terms themselves, and the abbreviations Q. Q. V., Q. E. D., (Quod quidem verum est. Quod erat demonstrandum,) with which demonstrations in each of the methods always close,* are of themselves quite sufficient to characterize the nature of their respective processes. Now the method of induction does not exactly coincide with either of these methods; but it much more nearly resembles that of synthesis than of analysis. For it goes on eliciting facts, bearing on a certain point, and comparing them together till it ascertain the relation of that point to others that are known, and when this relation is perfect in all its points, the induction is said to be complete; so, on the whole, the only difference between induction and the synthetic method is, that, in synthesis, each of the facts or positions from which we reason were formerly known; in the inductive method we discover them in our progress. But the fact is, they have mistaken the analytic method for the synthetic.

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* Prop. Geom. more veterum demon. Auc. Mat. Stewart.

As to the *invention* or discovery of abstract truth, we believe the analytic method to be much superior to the synthetic, and, in truth, the only one that can be followed successfully in these investigations; and in this respect *alone* is it connected with the method of induction. For the purpose of *teaching*, or the *traduction* of truth, it is neither better nor worse than the synthetic; but being much less manageable or convenient than the latter, is therefore much less employed. Accordingly, although M. Magendie has told us mournfully,

> "Il est triste, mais en même temps nécessaire de dire, q'au milieu de ce mouvement général des sciences, la physiologie, cette branche si importante des connoisances humaines, a conservé jusqu'ici sa forme systématique,"

it is the method that has always been, and probably ever will be employed. It is, in fact, a substitute for the same process of induction which we have shewn it to resemble so nearly, being employed in establishing truths that are *supposed* new to the reader, in the same way as induction is in those that are really so. But he goes on:

> "Qu'en est-il résulté? C'est que la physiologie, toute brillante qu'il paraisse dans les traités écrits de nos jours, et malgré les perfectionnemens que des hommes d'un grand talent ont cru lui faire éprouver, est encore une science à son berceau. Il faut absolument qu'elle sorte de cet etat affligeant d'imperfection. Pour obtenir ce résultat, le premier pas à faire doit être de changer la forme et par conséquent la marche qu'elle a suivie jusqu'ici; elle doit prendre la *marche analytique*, et la forme théorique: âlors seulement elle pourra se perfectionner.—*Mon but principal en écrivant cet ouvrage*, a été de contribuer à amener cet important changement; j'ai fait du moins tous mes efforts pour présenter la science sous la forme theorique, en suivant dans l'exposition des faits la *marche analytique*."

Yet his readers need not be under any apprehensions for the length and tedious caution of his experimental details, dry processes of exclusion,* comparison, and modification, such as a diligent admirer of Bacon must think himself obliged to pursue in his inquiries. They will find none of these, nor a single trace of the method of induction from one end of the book to the other; and, but for the preface, might have perused it most carefully without once discovering that such a method was known to its author. M. Magendie, unknown to himself, has acted much more sensibly; for, had he ever commenced in that method, he would soon have discovered it to be perfectly inadequate to his purpose of teaching. Bacon himself scarcely seems to have dreamed of employing it in the *traduction* or teaching of science, affirming, "perinde eam inter *desiderata* numerabimus, eamque traditionem lampadis sive methodus ad filios appellabimus." Indeed we shall close these remarks, into which the stale affectation of a method which does not at all belong to physiology has insensibly betrayed us, by recommending to M Magendie, and all who feel disposed to quote Bacon, a diligent perusal of the book we have been citing.

Our author's *Precis*, thus prefaced and arranged, goes over the different divisions with tolerable spirit, in language always clear and sensible; giving, on the whole, a pretty fair abstract of the science of the day. Maugre the warnings of his admired Bacon in the book above cited, (Augm. Sc. VI.) he ventures to amuse us now and then with his *questions* and *paradoxes*, especially on points on which all the world, except himself, seems agreed; † and, frequently, with his own views and experiments. Many of the latter are really instructive, and constitute the most valuable portion of his book. As manner is of infinitely less consequence than matter, we shall not stop to mention the many propositions which M. Magendie, forgetting his "*but principal*," has advanced without the necessary evidence,—the disproportionate attention and time bestowed on those in which he himself is any how concerned. Neither shall we notice the neglect of citing authorities, where these alone are the foundation of our belief; nor the national failing of never naming an author, "quin unà refutet;" ‡ and of some pretty manifest indications of a wish to appear a good Frenchman, by bringing the names of his countrymen in contact with as many discoveries and inventions as possible. We have said enough of his display of method already, and now proceed to present our readers with some of the principal observations which occurred to us on perusing the " Precis Elementaire."

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* Vid. Nov. Org. Lib. II. XVIII. et supra.

† Augm. Scient. Lib. VI. Cap. ii.

‡ Haller supra.

At the commencement he dwells at some length on the principles of our bodies and their technical arrangement, rather longer than is consistent with the plan of the work. " Divisions," says Bacon, " only give us the husks and outer parts of science: they allow the juice and kernel to escape in the splitting." Something, however, must be granted to the manners of the day.

The image that is formed by the condensation of the rays of light on the retina has been called in question by many; and a treatise so late as 1767 was published in England to disprove its existence. M. Magendie has instituted an excellent suite of experiments on this image, showing the way to exhibit it on the retinae of Albinos, (60,) such as white rabbits, white pigeons, white mice, which, from the fineness of their choroid and sclerotic coats, admitting the image to be reflected through them, are admirably adapted to this purpose. The general method was known to Haller, and had been tried by him and Malpighi * on the owl. But this application of it to the Albino variety of animals, is proper to our author, and, if not useful, is at least ingenious. At page 75, we find a curious experiment to prove the use of *two* eyes in judging of distances.

> " Suspendez à un fil un anneau, adaptez à l'extrémité d'une longue baguette un crochet qui puisse facilement entrer dans cet anneau ; placez-vous à une distance convenable, et cherchez à y introduire ce crochet: en vous servant des deux yeux, vous réussirez facilement à chaque coup ; mais si vous fermez un oeil, et que vous veuilliez enfiler l'anneau, vous ne réussirez plus ; le crochet ira au-delà, ou restera en deçà, et ce ne sera que par hasard, ou en tâtonnant long-temps que vous y parviendrez. Les personnes qui ont les yeux d'une force très-inégale, ne réussissent pas dans cet expériénce, même lorsquelles se servent des deux yeux."

The sensations described by the youth who was couched by the famous Cheselden, are generally considered by philosophers as decisive evidence of our primary ideas of *form* and *position* being derived from touch. (Hall. El. Phy. V. 518-520.) Our author recounts the narration, (84,) and deduces from it nearly the same conclusions. He supposes, however, that Cheselden in this instance did not destroy a cataract, but a membrana pupillaris; yet, though he says " *il y a tout lieu de penser*," he has not favoured us with his reasons for so odd an opinion;

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* El. Phys. V. 469.

and if he had read the page (298) of Cheselden's book immediately preceding this history, he would have found that he meant exactly the same thing by the term couching as our present operators.

The doctrine of *acoustics*, of itself sufficiently defective, is treated in a very general way by our author. It is perhaps nothing amiss to handle a subject of which our knowledge is imperfect, in a manner more brief than systematic writers have chosen; but we are conscious that M. Magendie carries this brevity to a fault in many instances. There are some actions of the human microcosm, which, in all probability, our feeble understandings will never be able perfectly to unveil; but which, nevertheless, are of such high importance to its existence and well being, that all mankind, whether learned or unlearned, frame to themselves some hypothesis of the way in which they are performed. *Hearing*, the *uterine secretion*, the *cerebral functions*, the office of the *aporous glands*, are of this class, and many others which are usually considered as physiological questions. Such inquiries do not indeed teach us what is, but they well instruct us in what is *not*; a knowledge which forms a very considerable part of many sciences partaking of that general advancement ("*mouvement general*") from which our author laments that physiology is excluded. In these, then, we cannot discover how the mere *ipse dixit* of any writer who chuses to proclaim the result uncertain, can serve to convey a sufficient knowledge of the many instructive investigations to which they have given birth, and interesting collateral laws of the economy thereby discovered; nor can we see what right he has to direct the attention of the student from speculations so instructive, by appearing to exonerate his mind from a trifling labour;—if that can be called labour, which, in the perusal of a long work, or the sameness of a long course of lectures, is rather a pleasing excursion.

The doctrine of smell (116) is another instance of this brevity; and the protest entered by our author against its accuracy (in a note) is scarcely a sufficient apology for the hardy hypothesis on which it is founded. It is not the doctrine of M. Magendie, but of the school of Paris. It concludes with one of the paradoxes to which we lately hinted our author is very liable,—that the olfactory nerve is not the sole organ of smell.

Taste is dispatched in a manner quite as summary as the senses just mentioned. We find, however, some novelties for which we were not altogether prepared. M. Magendie, it seems, has repeatedly failed (123) in attempting to trace the fifth pair of cerebral nerves into the papillae of the tongue; and from hence he concludes that they do not enter the papillae. Of course they are not the organs of the sense of taste. This is not a very correct way of reasoning; and occurs more than once with M. Magendie. He appears to forget that one affirmative is worth an hundred negatives; and that, in this instance, we have the direct personal authority of Haller (V. 104, 112. IV. 219) and Meckel, (97.) At same time, M. Magendie mentions an opinion of Miel, that the pain produced in the teeth by acids is attributable to imbibition ! a property of which that gentleman is said to have proved the very prompt exercise in the human teeth, (124.) Here we experience the loss of M. Magendie's neglect of quotation.

In delivering the doctrine of touch, our author takes an opportunity of distinguishing between tact and touch. The opinion is abundantly curious, though, it would appear, not at all original. (Bichat. Anat. Gener. IV. 711-714.)

According to M. Magendie, it appears that touch is tact united to the motions of the fingers. We have no great faith in the distinction; but a metaphysician of the modern Edinburgh school might found some curious speculations on it. We were astonished to find the discovery, at least the first accurate description of the papillae of the skin, referred to a M. Gautier ! We did not conceive that any one by having written the last "*monographie*" on a given subject, became thereby entitled to the discovery. Certainly all that he says about them was known a century before ; except that the cuticle is perforated at their summits, which it is hard to credit or prove, though natural to suppose. The causes of erroneous judgment of the heat and cold of external bodies, M. Magendie has briefly and neatly described. But he should not have said (132) it is the *capacity* of bodies for caloric (132) which influences our decision. This is far from being the case ; and we suppose he meant the *conducting power* of bodies.

Though prepared for novel views, we did not expect to meet a passage like the following :

> "L'intelligence de l'homme se compose de phénomènes tellement différens de tout ce que présente d'ailleurs la nature, qu'on les rapporte à un être particulier que l'on regarde comme une émanation de la Divinité. Il est trop *consolant* de croire à cet être, pour que le physiologiste mette en doute son existence ; mais la sévérité de langage ou de logique que comporte *maintenant* la physiologie, (*who, it seems, is got out of the* BERCEAU *in which she lay when our author wrote his preface*,) exige que l'on traite de l'intelligence humaine comme si elle était le résultat de l'action d'un organe. En s'écartant de cette marche, des hommes justement célèbres sont tombés dans de

graves erreurs ; en la suivant, on a, d'ailleurs, le grand avantage de conserver la même méthode d'étude, et de rendre très-faciles des choses qui sont envisagées généralement comme presque au-dessus de l'esprit humain."

How this "severity of language and logic" comes to be so much better understood in France than any where else, the author does not condescend to inform us ; and this neglect we regret most piteously, since we have always been too dull to comprehend how the exquisite operations of human intellect could be explained by the supposed action of an inert pulp, which, to common eyes, exhibits little more organization than a cocoa-nut or a bowl of butter. We are also at a loss respecting the nature and extent of the *graves erreurs* mentioned in the text into which those unfortunates, who believed that they had a soul to be saved, have fallen ; though, as such unscientific schismatics are happily still pretty numerous out of France, we can readily believe that some of them may have lapsed into serious errors. Till now, in spite of the most vehement craniologists, we had in our simplicity conceived that one might, without any great hazard, consider the mind as something within us purely intellectual and immaterial ; but now, it seems, in order to avoid these great errors, it is absolutely necessary to consider the human understanding "comme si elle etait le resultat de l'action d'un organe !" We shall proceed then in our course, which is to render "very easy, things which are generally considered as beyond the reach of human genius." The sequel, however, presents almost nothing uncommon or original, if we except the following propositions ; most of which, it is much to be feared, will still be considered as above the reach of human inquiry.

1. The brain is the organ of thought ; and this includes the brain proper, the cerebellum, and medulla oblongata. (155.) Accordingly, M. M. asserts that the brain is studied by anatomists for the importance of its functions. The brain is dissected, we believe, not so much because we do not know its functions, as because it is often the seat of disease, and is soft and sectile ; reasons which make students ashamed to be ignorant of its structure.

2. Blows change the form of the cranium and its contents ! (158.)

3 In general, the volume of the brain (to be measured by the cranium only) is in a direct ratio with the capacity of the mind. (163.)

4. It is worth considering whether the convolutions have any relation to the perfection of the mental faculties. (163.)

5. The sympathetic nerve sends branches to the brain. (167.)

6. Human understanding is only so many modifications of perception, (171,) which is subject to four modifications. I. Sensibility. II. Memory. III. Judgment. IV. Will, or the desires.

a. Sensibility, according to him, is whatever passes in the mind, it is, therefore, ideas or perception

b. He divides *memory* into memory strictly so called, and reminiscence; and for these distinctions there is perhaps some foundation.

7. Judgment, genius, and imagination go all in a lump, and bad reasoning depends on *organization.* Logic, however, is of no use; that is some comfort. (176.) "Il existe une science dont le but est d'apprendre à raisonner juste, c'est *la logique;* mais le jugement erroné, ou l'esprit faux, tiennent à l'organization. Il est impossible de ce changer à cet egard; nous restons tels que la nature nous a faits."

8. Generalization and abstraction are peculiar to man, *but only when in association with other men in society.*

9. Instinct is divisible into *animal* and *social;* the latter species only is manifested in the social state. The social instinct consists in a wish for a lively feeling of existence. Luckily, however, it is balanced by a wish for repose.

Such are the inestimable advantages to be derived from considering (155) the human understanding as the result of organic action. We do not think the reader feels himself much the wiser of them; and, for our part, we are, on the whole, sorry to find an author, apparently a man of sense, and manifestly of considerable talents, patronising this coarse remnant of materialism, which all the clumsy imposture and false reasoning, and forged anatomy, that has been advanced by the craniologists whom he has been weak enough to follow, will scarcely preserve in the memory of the public till a second edition of his physiology be called for.

We come now to consider M. Magendie's account of muscular action. This part of his work is well written, and presents a correct, though unsupported view of the subject: We say unsupported, because he does not offer any thing like proof of the principles he lays down. The question of the relative bulk of a muscle, before and during contraction, which has engaged so many eminent men, he dismisses by saying it is of no consequence. We should have preferred giving the student a cursory view of the many curious experiments instituted to ascertain it, and ultimately left it to himself to judge of the importance of the investigation. Authors should remember that mankind often derive more advantage from *luciferous* than *fructiferous* experiments, as Bacon remarks; and they see but a short way into nature, who can doubt that the former will not in time pass into the latter.

We find (194) a distinction made between the contraction of a muscle before and after death. The observation is curious, and seems to have escaped other physiologists. We do not find the reasons for distinguishing given in the text, an omission which we have too often to complain of, but it appears a plausible idea.

Proceeding to consider the functions of *Voice*, we observed it stated (202), that the anatomy of the organs of voice has not hitherto been carefully examined One would indeed conceive, *a priori*, that nearly 40 years of keen controversy might have brought to light a pretty accurate account of five small cartilages and their ligaments and muscles, which compose these organs; and M. M. must really furnish some better proof to the contrary, than that of transforming, as he does, a little cellular membrane into an *epiglottid* gland, before we can bring ourselves to assent to this startling proposition. As our author has furnished nothing to supply this defect in the anatomy,* so we cannot say that the physiology has been much a gainer by his labours. The idea of applying the *anche*, or reed, in some musical instruments, to explain the functions of the larynx, is perhaps original; and though it is liable to objections common to all mechanical explanations of living actions, yet it must be allowed to afford a very pretty illustration of the doctrine of voice. We must refer our readers to the part of the work itself, as it is much too long for transcription.

The experiment of inflating the dead larynx in man and animals (211) was familiar to Haller, to Ferrein, and a host of other physiologists. Hall. El. Phys. III. 437, 438. Indeed, the theory of Magendie is precisely the same with that delivered by Haller in his immortal work.

There follows (219) an account of the effects produced on the voice by changes on the length of the trachea. It is exceedingly ingenious, and though entirely hypothetical, we should have extracted it, had we not recollected that Dodart long since (Hall. El. Phys. III. 455) advanced the same doctrine, and without making converts.

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* The connection of the cricoid and thyroid cartilage (202) which he supposes not generally known, is described by Haller, III. 384, and after him by Mr Fyfe, Anat. II. 164.

An art, long believed to be imaginary, but now practised by every strolling juggler, is, in reality, the result of operations of the larynx, though vulgarly named *Ventriloquism.* Physiologists have, accordingly, attended to it; and the following account of ventriloquism (235) from the city of ventriloquists, may not be unacceptable to our readers.

> "Les fondemens sur lesquels repose cet art sont faciles à saisir. Nous avons instinctivement reconnu, par l'experience, que les sons s'alterent par plusieurs causes: par exemple, qu'ils s'affaiblissent, deviennent moins distincts, et changent de timbre à mesure qu'ils s'eloignent de nous. Un homme est descendu au fond d'un puits, il veut parler aux personnes qui sont à l'ouverture; sa voix n'arrivera a leur oreille qu'avec des modifications dependantes de la distance, de la forme du canal, qu'elle a parcouru. Si donc une personne remarque bien ces modifications et s'exerce à les reproduire, il produira des illusions d'acoustique, dont on ne pourra pas plus se defendre, qu'on peut ne pas voir les objets plus gros lorsqu'on les regarde a travers un verre grossissant; l'erreur sera complète s'il emploie d'ailleurs les prestiges convenables pour detourner l'attention.
>
> "Plus l'artiste aura de talens, plus les illusions seront nombreuses; mais il faut se garder de croire qu'un ventriloque produise les sons vocaux et articule autrement qu'une autre personne. Sa voix se forme à la manière ordinaire."—'Sous un certain rapport, on peut dire que cet art est à l'oreille ce que la peinture est pour les yeux."

M. Magendie, however, does not appear to be in complete possession of the secret; and though we ourselves are no conjurers, we can assure him that nearly as much of the *prestige* depends on an axiom of acoustics, as on the accurate imitation of the change of sound produced by mere distance. The artist always knows that the ear judges most imperfectly of *the angle of direction* of the body from which the sound originates, so as scarcely at all to perceive a very considerable deviation; and, accordingly, he turns his face and gestures in *that direction* from which he designs the audience to conceive that the sound proceeds. The audience, always at some distance from the performer, have their eyes and ears unconsciously directed towards this point; and, as the latter are unable to distinguish the small variation of angle which a line drawn from the performer to it makes with their own direction; and as he can, by imitation, make up for the effect of *distance*, there is evidently no other means left by which the audience can detect the illusion, or the ear judge of the true situation from which the sound issues.

Sixty five pages, at the end of this volume, are occupied with explaining *Muscular Motion.*

If we understand M. Magendie aright, he considers *muscular motion* as being under the power of the will; but that the *contrac-*

tion of the muscles necessary to produce it is not under the influence of that cerebral action ; it is purely instinctive, (305, 306.) Now he defines instinct (179) to be "*propensities. inclinations, wants* ;" so, according to him, a muscle moves because we *wish* it moved, that is to say, it contracts from *volition*, which is just the common opinion, but, as an explanation, must go for nothing. That a long, though rapid series of unknown actions may, and probably *does*, intervene between the volition and the ultimate contraction of a muscle, is what nobody doubts ; but there does not appear to be any good reason for concluding, from such negative concessions, that the *will* has her residence in the brain and cerebellum, whilst *instinct* sits snugly pent up in the cavity of the spinal marrow, as the direct cause of motion, (306.) Even the experiments of Le Gallois and Wilson Philip are far from proving so much.

The general character of the second volume is of a higher order than the first. It embraces objects more within the reach of experiment ; and, accordingly, in the discussion of these, we are always pleased to find the author rich in observation and experimental knowledge, often adding to the labours of others, and always exhibiting clear and novel views of what has already been performed.

The whole of the section on digestion is excellent. Here and there we meet with inaccuracies and discrepancies, many of which, from the present unfortunate practice of neglecting references, we are quite incapable of explaining. Thus we are told (47) that 99.5 parts of 100 in human teeth consist of salts ; the rest (.5) alone being animal matter ; an account by no means consistent with the analysis usually received in this country.

From page 76 to 86 is found an ingenious and detailed explanation of the mode by which the alimentary mass is transferred from the stomach to the intestines ; but much hypothesis is mixed with observation. We would, nevertheless, recommend it to our readers as the best yet offered. We find M. M., of course, rather severe on Haller's Theory of Maceration, a process by which that eminent man attempted to explain digestion. Magendie himself admits pressure, (90.) He finds, (90,) what was always suspected, that *artificial digestion*, i. e. alimentary matters mixed with the gastric juice, and exposed to the temperature of the stomach, does not at all succeed in imitating the concoctive process of the organ. This he asserts on the authority of Montègre, who thereby overthrows the experiments of Spallanzani and Reaumur. M. Magendie seems either not to have heard of, or unaccountably neglects to mention the experiments of Stevens, whilst repeatedly alluding to the above philosophers.

Physiological authors, it seems, have usually treated of the changes which meat and drink undergo in the alimentary canal as similar, and describe them under the same head. Our author sets them right in this particular, and furnishes us with an excellent and detailed account (117 to 130) of the changes encountered by fluids in their passage through the stomach and intestines. Many of these facts are abundantly curious, and, moreover, point out practical considerations of the first importance. Thus, he finds, (119,) that fluids, in whatever quantity introduced into the stomach, disappear most rapidly, and that a *ligature* on the pylorus does not impede this sudden disappearance. Alcohol (128) produces many curious effects; it augments the mucous secretion, produces contraction of the stomach, coagulates the albumen, of which it always contains a plentiful portion, and is afterwards absorbed with great rapidity. If it contained foreign matters in solution, these are respectively separated from it in the stomach, and undergo, without disappearing suddenly, the same process with the solid aliment. *But salts are not separated,* and vanish as quickly as their menstruum. The same thing is true of salts and esculent matters held in solution by WATER. Thus *wine* and *soup* are decomposed; the fluid part evanishing so quickly, that seldom if ever is it found to arrive so far as the ilium; whilst the solid parts, separated from their menstrua, are left to be leisurely submitted to the general process of concoction, (130.) This is one of the many instances where the vulgar are found to be more in the right than philosophers; and the expressions, of wine, porter, and similar fluids having a BODY, as it is called, appear quite well founded. We recommend the consideration of the above facts to every practitioner.

The world is long since in possession of M. Magendie's series of experiments on vomiting. His conclusions, however, do not seem to be so well understood; and we ourselves were not prepared to discover, as we did, that his theory (140) of that operation is nearly the same as Haller's, or as that which has been received in this country for many years. But Magendie is wrong in supposing (140) that Haller considered *contraction* of the stomach to be the essential cause of vomiting.

In describing the chyle, a very precise account is given (155) of the mode in which that fluid is obtained. From a dog about six ounces may be procured in the course of an hour. The whole particulars relating to this fluid, though containing little new, are delivered in a very distinct and impressive manner. He finds, however, that colouring matters introduced into the intestine do not impart their colour (157) to the chyle. Although M. Magendie's experiments on vomiting are well known in this country, they have by no means made so much noise as those he has published on the lymphatic system. These go far, as he himself thinks, to overthrow the absorbing power usually ascribed to the above organs; and to restore it to the venous system, which the present generation has almost forgot to have ever been allowed its possession. His arguments for the latter, the absorbing faculty of veins, are so forcible that we do not see how their obvious result can be controverted; but the proofs adduced against the same property being possessed by lymphatics, are generally of the negative class, and of a much less satisfactory character. Indeed, the result seems to have made a similar impression on the author himself. We have only room to trace briefly an outline of the arguments offered.

I. Lymph is a term used in a very indeterminate sense, and this circumstance has become a frequent source of error, (170, 171;) but lymph, properly speaking, is that *opaline*, *rose-coloured*, sometimes *madder-red* fluid, which is obtained by puncturing the lymphatics or the thoracic duct after a long fast. From not attending to the colour above mentioned, M. Magendie is of opinion, that many mistakes have originated respecting the entrance of coloured matter into the lymphatics. No fluid similar to lymph resides in the cavities; the lymph being similar to itself every where over the body, whilst the fluid of cavities is perpetually varying, (177.) Indeed, we are not aware that any one ever maintained this opinion. Dumas certainly did not, being at pains to explain the contrary.

II. The art of making physiological experiments was imperfect in the time at which John Hunter performed his, wherein he denied venous absorption, and asserted that of the lacteals. This is the only thing can excuse this great man for not having perceived how many circumstances were awanting to render them decisive.

In this country, it seems hard to believe that much advancement has been made in the above respect. The most ingenious man, and acute anatomist of his age, Hunter, at length arrived, says his biographer, at the enviable degree of operative accuracy, in which the hand can execute whatever the head has devised. What it is that " the art of making experiments" can furnish, at all equivalent to these qualifications, we are too dull to comprehend.

III. Hunter's experiments do not succeed with MM. Magendie and Flandrin, not even those in which indigo water injected into the peritoneum became afterwards visible in the absorbents.

We stated above, that one successful experiment should weigh against a great many negative trials; and since Musgrave, Lister, and Haller, Kauw Boerhaave, W. and J. Hunter have succeeded, it is probable that the more cautious part of physiologists will still turn a sceptical ear to the one hundred and fifty unfortunate attempts (187) of MM. Magendie, Flandrin, and Dupuytren.

IV. Membranes that have no lymphatics demonstrable, as the arachnoid, nevertheless are found to absorb actively, (188.) Here our author forgets that such membranes, according to Bichat, have not their veins, to which he attributes absorption, more conspicuous than their lymphatics. Veins are seen to creep on them, but never to enter. (Bich. Membr.)

V. The appearances supposed to confirm lymphatic absorption may be explained another way, according to our author. Thus, (185,) after removing the glands of the axilla, in the operation for cancerous mamma, it frequently happens, that the arm swells, and serosity is accumulated in the cellular tissue. This is very naturally referred to the destruction of the passage of that fluid to its thoracic trunk, which takes place by the axillary glands, through which they almost all pass, being forcibly torn away. But M. Magendie will not admit of this, especially as the above effect is not always produced by this cause, and very seldom by disorganization. This is far from being satisfactory.

The well known and fatal effects of a lymphatic being inflamed by a wound from a point imbued in acrid matter, is a fact which presses our author very hard. He is not altogether disconcerted, however, and suggests a number of curious objections (190) to the most obvious conclusion.

Swelling of the inguinal glands after syphilitic or gonorrheal contagion, has usually been received as an irrefragable proof of absorption taking place by lymphatics. Magendie maintains, " Ces differens faits, sont, il est vrais, de nature à faire *soupçonner* l'absorption par les vaisseux lymphatiques, mais ils ne la demontrent certainement pas." A case of Dupuytren's is quoted, in which pus was seen in the lymphatics, (194,) just as other people out of France have seen it times innumerable; but our author, as above, stands on the identity of the pus in the lymphatics, with that in the tumour from which it was supposed to be taken up, not being established. This appears captious in the extreme. If the chyle is one thing in the intestines, another in the lacteals, and a third in the great duct, as according to his way of reasoning its different appearance would lead us to conclude, how shall we be able to trace it to the heart and circulation? Yet the above mentioned way of reasoning would infallibly lead us to this very absurd conclusion, of denying the identity of the chyle in those different steps of its progress. Indeed, it is much more wonderful, that an animal fluid like pus should retain its chief qualities unaltered after absorption, than that it should be somewhat changed. Yet, after all, the dissector of the above case declares, " I have denominated this fluid pus, because it had the *opacity*, the *white colour*, and *consistence* of pus," (195.) There is more probability in surmising (194, 195) that the pus may have come from the vessel itself being inflamed, though this is merely one conjecture brought to support another, and directly opposed by the fact of the vessel being enlarged.

To make his hypothesis complete in all its parts, our author suggests that the lymphatics are continuous with the extremities of the arteries.

Following our author into his account of the blood, the usual analysis occurs; and we find him stating, as some do now in England, that the coagulation of that fluid demonstrates it to be possessed of life. The argument applies to milk, and the elements of soap before their commixture, with equal cogency.

Assisted by the microscope, our author (208) finds, in fresh drawn blood, nearly the same appearances as those described by Sir Everard Home; * and by him supposed to explain the formation of vessels in new parts. Of these observations we have given some account in a former Number; † and shall, therefore, only remark here, that the observations of Magendie afford a most conspicuous confirmation of those of Sir E. Home, though they could hardly have suggested them.

Our author, it appears, has some pretensions to chemistry, and some analyses of his are among those received by the oracles of that science. It may be interesting, therefore, to our readers to know, that though he denies the muscularity of arteries and veins, yet he suspects, from experiment, that the middle coat, both of the veins (215) and the pulmonary arteries, (220,) consists of fibrin.

The subject of venous absorption, as was to be expected from his views of the office of the lymphatics, has been successfully laboured by M. Magendie. Haller, and many others, have shewn by positive experiments, that the veins absorb, and scarcely any other opinion is to be met with prior to the time of W. and J. Hunter. These illustrious anatomists, the glory and boast of the country that gave them birth, conceiving that the lymphatics alone are capable of performing their office unassisted, were inclined to deny it to the veins; and J. Hunter, from eight experiments,‡ which, according to our author, were all very faulty, (241,) believed that he had demonstrated their incapacity for absorption. We do not reject altogether the objections to them here advanced, but we think the chief source of error was in John Hunter's expecting to meet with water or milk in the veins, possessed of the same qualities as that which he had thrown into the intestine. Had he found them thus, the proof indeed would have been complete; but their *not* being found, is very far from being an argument that they were *not* absorbed by the veins. As proof of this power existing in veins, our author gives the following very satisfactory experiment.

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* Phil. Trans. 1818. † Vol. XV. p. 118. ‡ Med. Com.

> “ M. *Delille* et moi, nous separâmes du corps la cuisse d'un chien, assoupi précédemment par l'opium (afin de lui eviter les douleurs inseparables d'une experience laborieuse;) nous laissames seulement intacts l'artere, et la veine crurale, qui conservaient la communication entre la cuisse et le tronc. Ces deux vaisseaux furent disséqués avec le plus grand soin, c'est-à-dire qu'ils furent isolés dans l'etendue, de quatre centimetres; leur tunique cellulaire fut enlevée, dans la crainte qu'elle ne recelât quelques vaisseaux lymphatiques. Deux grains d'un poison tres-subtil (l'upas tieuté) furent alors enfoncés dans la patte; les effets de ce poison furent tout aussi prompts et aussi intenses que si la cuisse n'eut point été separée du corps; en sorte qu'ils se manifesterent avant la quatrième minute, et que l'animal etait mort avant la dixieme.
>
> “ On pouvait objecter que, malgré toutes les précautions prises, les parois de l'artere et de la veine crurale contenaient encore des lymphatiques; et que ces vaisseaux suffisaient pour donner passage au poison.
>
> “ Pour lever cette difficulte, je repetai sur un autre chien l'experience precedente, avec cette modification, que j'introduisis dans l'artere crurale un petit tuyau de plume, sur lequel je fixai ce vaisseau par deux ligatures; l'artère fut ensuite coupée circulairement entre les deux ligatures, j'en fis autant pour la veine crurale: par la il n'y eut plus de communication entre la cuisse et le reste du corps, si ce n'est par le sang arteriel qui arrivait à la cuisse, et le veineux qui retournait au tronc. Le poison, introduit ensuite dans la patte, produisit ses effets dans le temps ordinaire, c'est-à-dire, au bout d'environ quatre minutes.
>
> “ Cette experience ne laisse point douter que le poison n'ait passé de la patte au tronc à travers la veine crurale. Pour rendre le phenomene encore plus evident, il faut presser cette veine entre les doigts au moment où les effets du poison commencent à se developper; ces effets cessent bientot; ils reparaissent des qu'on laisse la veine libre, et cessent encore si on la comprime de nouveau. On peut ainsi les graduer à volonté.”

We think this experiment so far decisive: yet must own that we are prevented from granting it unalloyed confidence, by considering that the act of burying the poison in the paw of the animal must necessarily penetrate many of its plantar veins, and of course introduce into them a portion of this very subtile poison, (upas,) which will thus be brought by them into the crural vein, in which they terminate. It will be no greater wonder, in such a case, if the animal die by this operation, than if it were destroyed by directly injecting the poison into its veins.

His other arguments are,

2 Flandrin finds the venous blood of different odour and qualities in different veins; and of an odour analogous to the fluid of its respective intestine; whilst no smell connected with the food can be observed in the vessels that carry chyle

3 Half a pound of assafœtida was given to a horse, which being killed sixteen hours thereafter, gave a distinct smell of assafœtida in the veins of the stomach, of the small intestines, and of the cœcum; the same smell was not at all remarked in the arterial blood, nor in the lymph

4. Camphor and other odoriferous substances, on being introduced into the cavity of the chest or rectum, are after a few minutes discharged from the lungs, (230.)

5. Odoriferous substances introduced into the intestine, are soon distinctly recognised in the branches of the vena portæ, long indeed before any trace of them is to be observed in the lymph.

6. Bile, or atmospherical air, injected into the crural vein, rapidly prove fatal. The same substances, introduced into the porta, do not produce the least apparent inconvenience.

7. The *invertebrata* absorb by veins, having no lymphatics.

These positions, supported by abundance of experiments, are very strong; and we think, that, taken *in cumulo*, they go far to establish the existence of absorption in the animal body by means of veins. It is not likely that any great change of opinion can take place in the theory of a capital function of the human body, without producing some practical inference of importance. In this instance, if it be made out that matters pass directly from the intestine by the porta, our confidence in those substances, which Durande and others have proposed as solvents in biliary calculi, must be increased in an eminent degree.

The doctrine of *pulmonary circulation* is delivered by our author with tolerable interest; but, excepting a few remarks on the pulmonary artery, it affords nothing to arrest our attention. On *respiration*, (268,) he ventures to differ from Haller toto cœlo in the mechanical explanation of that function; and to attribute inspiration chiefly to the *scaleni* and similar muscles arising from the *neck*; not to the intercostals, which he thinks (272) are quite unable of themselves to perform the function of raising the ribs. His arguments do not appear to us to be very cogent, and are certainly too tedious to be here extracted, so we refer the reader to them in their place. At any rate, he sets out with false principles; for, certainly no one but himself ever thought of measuring the mobility of each rib at an equal distance (269) from the centre of motion. We dispute about the elevation of the ribs, only because the augmentation of the capacity of the thorax is commensurate with it; and this increased capacity is always measured by the motion of the *middle* of the rib, and not of an imaginary point, situated at a given number of inches from its extremity.

In the chemical part of respiration, which is very well conducted, we find the author maintaining, that, by the last experiments of Gay-Lussac and Davy, there is a little more carbonic acid formed than oxygen consumed. He then gives us an useful table of the differences between venous and arterial blood, as follows:

	Venous Blood.	*Arterial Blood.*
Colour,	Reddish brown.	Vermillion red.
Smell, . .	Faint.	Strong.
Temperature,	101°.5 F.	103° F. ferè.
Capacity for caloric,	852.*	839.
Specific gravity,	1051.	1049.
Coagulation,	Less rapid.	More rapid.
Serum, . .	More abundant.	Less abundant.

Our author has applied the aqueous injections of Stephen Hales (Haemostatics, II. 75, 74) to the investigation of pulmonary transpiration, and relates (291) some very interesting results. Alcohol, ether, camphor, the gases, are all, by his experiments, and those of Nysten, found readily to transpire by the lungs.

From 298 to 301 is a good, though not quite original, detail of the effects arising from a division of the 8th pair of nerves. At 309, 321, he ridicules, with great propriety and effect, the notion of a peculiar automatic action of the capillary vessels. We are astonished that *practical* men should still, occasionally, continue to amuse themselves with conjectures so utterly incapable of confirmation. At the same time, M. Magendie takes an opportunity of exposing the weakness of Bichat's favourite doctrine, that the circulation in an organ is incapable of alteration from change of its density, or the form of its vessels. Another of Bichat's notions was, that the blood in veins is moved entirely by the force of the action proper to the capillaries. Magendie refutes this opinion by an experiment so simple, that it admits of no reply.

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* Dr Davy instituted four different series of experiments to determine the relative capacities of these fluids. The first gave Art. 934, Ven. 921; the second, Art. 814, Ven. 812; the third, Art. 913, Ven. 903; and the fourth set alone, and probably the least correct, (Thoms. I. 117,) exhibits the proportions here given by our author, Art. 839, Ven. 852. So little decisive is obtained from Davy's experiments. Haller did not believe that any radical difference of arterial and venous blood existed.

Exposing and liberating the crural artery and vein of a dog, the rest of the thigh was included in a ligature. Then a small separate ligature was applied on the vein near the groin, and the vein was now punctured below it. On compressing the artery between the fingers, blood flowed, as before, from the puncture in the vein for a time; but soon stopped, though the vessel was still quite full. On letting go the artery, as soon as ever the blood arrived at its extremity, the jet from the hole in the vein, beneath the ligature, recommenced with violence.

After a short account of transfusion, in which we find our author somewhat disposed to transfer the merit of its discovery to France, or at least to name nobody but Frenchmen, (341,) he proceeds to consider the different *secretions.* Here is found little new. The author, as is usual on this subject, is only successful in combating the opinion of others; but that he might not leave us totally in the dark, informs us that M. le Professeur Chaussier has denominated the glands not known to secrete (as the spleen) *glandiform ganglions!*

On the subject of nutrition, M. Magendie recurs to a doctrine he elsewhere maintains, of the molecules which form our bodies being permanent, and not undergoing the perpetual change and fluctuation which is vulgarly ascribed to them. That such a change and renovation actually takes place, is a natural consequence of the body receiving a daily supply of nourishment; and we do not see that M. M. has brought forward a single argument which tends to establish his opinion.

The author takes an opportunity, in this place, of detailing his very curious experiments on the effects of feeding animals naturally carnivorous, on substances not containing azote, as sugar, oil, butter, &c. The results of these he has lately given, under a more practical point of view; and, as they have been considered in the last Number of this Journal, cannot, with propriety, be here repeated. M. Bres's doctrine of the generation of heat by flexure of the limbs, is admitted and applied, (400.) He also establishes a fact rather curious, that the heat of the human body is, in reality, capable of being raised somewhat (7° F.) above the natural standard, by means of external heat. The experimentalists, to whom we owe this, (Delaroche and Berger,) have likewise afforded a most convincing proof of the beautiful observation first made by Franklin, that the uniformity of human temperature, from which we have just recorded so small a deviation, is actually owing to evaporation taking place from the different surfaces. Confining animals in a chamber whose atmosphere was so moist that no evaporation could take place from their surface, it was found that their temperature was as steadily and uniformly raised, by increasing the heat of the room, as if they had been inanimate matter: "Comme s'ils n'avaient plus aucun moyen de se refroidir." (403.) Some experiments by Drs Edwards and Gentil are likewise recorded, to prove that thermometrical heat is not exactly the same in different individuals of the human race in different temperaments, and even at different times of the day. All this is very curious, and should be confirmed by further trials.

On the doctrine of *generation*, we find much interesting remark and curious suggestion, alloyed, however, now and then, with a disposition to cut the knot, where the ingenious author, like other authors, finds himself unable to untie it.

This work, on which we are afraid we have tired the patience of all but physiological readers, concludes with a few observations on sleep and death.

We come now to conclude these remarks on a work which, though it certainly does not meet the high expectations we had unwarily formed, is, nevertheless, a most valuable production; and constitutes an imposing earnest of what may be expected from this writer, when time shall have rendered familiar that flash of novelty which yet seems to dazzle him in his own speculations. Time, and the opposition which rising merit never fails to encounter, will teach him a purer logic, and a fairer mode of estimating the facts and arguments which operate against his own doctrines; but they can hardly add any thing to the ingenuity and diligence to which we already owe so much, and which, aided by such auxiliaries, must prove equally beneficial to physiology and ornamental to his country.

II.

Practical Observations on Fever, Dysentery, and Liver Complaints, as they occur amongst the European Troops in India; with Introductory Remarks on the Disadvantages of selecting Boys for Indian Military Service. Illustrated by numerous Tables and Cases. By George Ballingall, Esq. Surgeon to his Majesty's 33d Regiment of Foot. Edinburgh, 1818. pp. 248. 8vo.

The author of this work informs us that it was written during a voyage from Madras to England, when a recent and extensive acquaintance of seven years with the diseases of

India had strongly impressed his mind with the peculiarities they exhibited. His library was extremely circumscribed, and he confined himself solely to the results of his own experience, referring occasionally to living practitioners, and to unpublished documents, rather than to works of more general and easy access. This is the sort of book we want,—a perspicuous detail of the phenomena of disease, and of the operation of remedies, given in plain unpretending language, unfettered by the theories or fashionable doctrines of the day. Sterne, or some other wit, has asked, "when were we to cease making books as apothecaries composed mixtures?" To judge from our annual catalogues, that happy period has not yet arrived. It must, however, be allowed, that, amidst a number of clumsy compilations, original and important works now and then appear; and, if there is one class of men more than another to whom we have of late been indebted for these productions, it is that to which Mr Ballingall belongs. Indeed, we cannot conceive a situation in which a conscientious physician of even moderate talents, can be placed, more favourable for observation than a military hospital, as at present conducted; and we hail with great pleasure, the opportunity of introducing to our readers a work so well calculated as Mr Ballingall's, to give a faithful picture of some of the diseases of India,—not as they have presented themselves among casual visitors at sea-ports, or along the coast, but as they have been observed and treated, both among new comers and seasoned residents, in the hospitals and camps of that country.

Mr B. sets out with some observations on the selection of troops for military service in India. As we wish to come rather to his remarks on the treatment of disease, we shall only state here, that in our opinion he has very clearly made out his position,—that it is a great error to send this description of persons on that service, and that a proper selection of troops for foreign and unhealthy climates, is a matter of much more importance than is generally imagined. The returns in his appendix, illustrative of this point, together with his remarks on them, we would strongly recommend to those concerned in such selections, whether medical or military officers; we shall merely state, that in the second battalion of the Royal Scots, in which Mr Ballingall served, and which was composed of a large proportion of youths and boys, upwards of three-fourths of the deaths, during their first year in India, were of persons under 25 years of age. In the six following years, they amounted to four-fifths; and during five months that a wing of the regiment was in the field, nearly six-sevenths of the deaths were among those of the same period of life. Well might the feelings of the medical attendants be excited at seeing so many youths "falling victims to a climate to which they ought long have been strangers, and to an opinion of their peculiar aptitude for service in that climate:"—they did indeed "fall without their fame in the first of their fields."

The part of Mr Ballingall's work which we mean more particularly to notice, is occupied with practical remarks on Fever and Dysentery. Of these we shall give a brief outline; but to those interested in the diseases of India, we would advise an attentive perusal of the work itself, and especially of the tables, cases, and dissections in the appendix; they admit of no abridgment, and they are essentially necessary to the illustration of the author's opinions and practice. One of the appendices, the 5th, is particularly valuable; it gives a comparative view, in a tabular form, of the sickness and mortality among the British troops at fourteen of the principal stations of the Madras presidency.

We shall not detail the well known symptoms of the diseases treated of, but shall confine ourselves to the method of cure; and first of that followed in Fever. He lays great stress on the vital importance of an early application of remedies in this disease; indeed, he professes this to be the leading object of his book.

> "The object of the following paper, therefore, is, to recommend the adoption of a practice in the early stages of fever which the urgency of the symptoms would not perhaps seem to demand, to press upon the attention of practitioners the necessity of a vigorous use of evacuations at the commencement of disease, and to urge the liberal use of purgatives, bloodletting, and the cold affusion. The two first, particularly adapted, in my opinion, to prevent visceral congestion and ultimate obstructions, so much to be dreaded as the sequelæ of this disease; and the last, calculated, when early employed, to put a speedy termination to its progress; and to prevent its fixing upon, and ever afterwards impairing the functions of important organs." p. 19.

Of the propriety of this plan of cure it is quite sufficient to state, that, during a period of nearly seven years, the Royal Scots lost in India only forty-two men by fever, and that of eighty-seven cases of that disease of which Mr B. kept notes, *eighty-six* terminated favourably, and the fatal case was complicated with liver disease. This is a success altogether decisive, and greatly exceeds the most fortunate averages in our European civil establishments. But we must make every allowance for the subjects treated in these hospitals, for among them are included all ages and constitutions, from infancy to old age, received at all periods of disease, and especially in its advanced stages. There is another circumstance, however, which should be taken into consideration in favour of the military hospitals, and which often swells their apparent mortality in fever; it is this:

> "Men, when admitted with prominent symptoms of fever, are naturally entered in the Hospital Register, under the head of that disease; and when death ensues, it sometimes happens that they appear on the returns as dead of fever, when, perhaps, they ought, strictly speaking, to be returned dead of dysentery or liver complaints. An apparent increase is thus made to the fatal list of fevers, and a proportional diminution from the mortality produced by dysentery." p. 26.

Upon the whole, it would appear that Fever is not the most prominent disease with which our armies in India are affected. In those who have died in consequence of it, and in whom Mr B. has had an opportunity of performing dissections, the brain, liver, and spleen, appeared to be invariably the seats of an increased flow of blood.

We shall now turn to the remedies used, taking them in the order laid down by the author.

1. Purgatives.—This is a class of remedies, peculiarly adapted to the fevers of India, in which costiveness is generally prevalent, as well as to most of the diseases of that climate; and until natural evacuations take place, no limit should be put to their administration. Mr B. states, that he has not found the preparations of calomel by any means so peculiarly appropriate as many seem to imagine. Useful as they may be, by the smallness of the volume in which they can be administered, they are, according to his experience, more transitory in their operation, and more apt to be followed by a return of constipation, and a disposition to the re-accumulation of fecal matter, than some other medicines, the Pulv. jalap. comp. for instance, or the sulphates of soda and magnesia. Whatever medicines are used, should be assisted by purgative enemas. The daily exhibition of neutral salts, purgative mixtures, or artificial mineral waters, should be persevered in until the febrile symptoms, and all sense of tenderness or other indication of abdominal congestion, have totally disappeared.

2. Bloodletting.—If the European physician were to form his judgment of the utility of this remedy from the productions of those authors who "direct physic and regimen by latitudes," or "those medical magicians who can sit at home, tell the transactions of every clime, and traverse the Hygeian zodiac with omnipotent skill," he would view this powerful remedy with invincible horror. Even under the guidance of Clerk and Curtis, authors well acquainted with India and its diseases, and whose works are in the hands of all who practise in that country, he would rarely, if ever, unsheath the lancet. How far the practice recom-

mended by Mr Ballingall differs from that laid down by these authors, our readers will best judge from the following extract.

"*Bloodletting.*—Of the objections existing in India to an active system of depletion in the treatment of disease, and of the amount of fortitude necessary to overcome these objections, no one can form an accurate estimate, until he has seen how far they are countenanced by the bloodless aspect, the emaciated figure, and languid movement of many of the older residents in that country. An extreme aversion to the use of the lancet is a trait remarkably conspicuous in the practice of the older Indian physicians. An aversion so generally diffused, and so firmly rooted, is not to be denominated prejudice, and consigned to disregard, particularly when we consider the class of men among whom it prevails—men of liberal and enlightened minds—men of the first education—and men grown grey in experience; and although many of these physicians are now elevated by their rank from the laborious duties of an European hospital, most of them have, in their younger days, had an ample share of intercourse with sick Europeans; and, having devoted their whole attention to the diseases of a particular climate, their observations on the diseases of that climate must be entitled to the first consideration. If their opinion of the general inutility of bloodletting is to be successfully confuted, it must be by patient investigation, and a detail of experience as laborious as that which has led to its adoption. To this fund of experience I can contribute but little. I may, however, safely assert, that neither in fever, nor in any other disease, have I ever had occasion to regret the employment of bloodletting; while, on the other hand, I have frequently had to blame myself for its omission. Instead of confining the use of this evacuation to the most urgent cases of fever, where its use appears indispensable, I should be inclined, in a great measure, to reverse the rule, and to practise it in every case where some peculiar circumstance does not forbid its use. Youth, plethora, vigour, and a recent arrival from Europe, are circumstances, which, in the opinion of many of our Indian practitioners, will alone justify the employment of this evacuation in any disease, and are circumstances in which I should find many ready to acquiesce in its utility; but it appears to me, that, although patients of this description will undoubtedly bear bleeding to a much larger extent than men of an opposite description, yet this evacuation becomes no less necessary in older men, to obviate the tendency of the disease to fix upon and impair the functions of the liver, from which an increase of years certainly affords them no exemption, and to which their residence in India, if of any duration, and the habits of the soldiery there, as certainly create a positive predisposition. We do not here, in general, require that copious detraction of blood which is necessary to arrest the progress of an active and violent inflammation of any particular viscus, but a more moderate depletion, calculated to prevent rather than remove a very general and widely diffused tendency to congestion; for this purpose, the abstraction of from 12 to 20 ounces of blood will frequently prove sufficient, if combined, at the same time, with the employment of purgatives. To this extent I have been in the habit of employing bleeding pretty frequently in the fevers I have had occasion to meet with in India; and to this I attribute, in a great measure, the very general exemption of my patients from those visceral obstructions so often consequent on this disease. The practice, indeed, has not been peculiarly mine, but has been very general in the regiment in which I have served. Where the violence of the headach, the flushing of the face, and suffusion of the eyeballs, point out the head as more particularly affected, in addition to a general bleeding, the opening of the temporal artery, or the application of leeches to the temples, has generally been practised with success; and, in one or two such cases, I have taken the blood from the external jugular vein. Where any particular tenderness over the region of liver has marked an incipient affection of that organ, blisters have been the local application more generally resorted to. The obscure nature of the functions of the spleen, and its comparative insensibility to pain, have seldom rendered a particular affection of this organ obvious or urgent in the active state of fever, but, should it in any case become so, blisters are here likely to be attended with similar advantages as in affections of the liver." pp 29-32.

Among the many subjects which, in our present advanced state of medical knowledge, loudly call for a critical inquiry, this of bloodletting in fever appears to us to be one of the most important. Nothing short of determined blindness can prevent us from acknowledging its actual utility at present; for it is too powerful and too frequently employed to admit of our supposing that nature barely *tolerates* it. But, it may be asked, Are we authorized to stigmatize our predecessors with the accusations of ignorance, blindness, or prejudice? Have they been led away by a fashion in physic, or have they acted from a comparison of mortality? Can it be that they treated diseases different in their nature, and occurring in constitutions of another tone and character from what we now meet with? Or, is it possible that, in the revolution of diseases and of constitutions, future physicians may throw upon our own practice the blame of inefficiency, which some of our less considerate brethren have heaped with no sparing hands on that of our fathers? That some of the writers on the diseases of hot climates were well acquainted with the powers of venesection in fever, and most judiciously discriminated the periods and stages of the disease in which it was a bane, and in which an antidote, will very clearly appear by referring to a book which we look upon as first rate authority upon the subject, the work of Hillary, (Diseases of Barbadoes, p. 156;) but we must candidly confess our incompetency, from any materials at present within our reach, to decide, or even to form a correct judgment upon the heads of inquiry which we have just thrown out. We know of nothing more likely to supply this defect than a publication from time to time under the auspices of the heads of the medical department of the army, both in India and at home, containing the results of the trials of various modes of treatment adopted in the hospitals under their control, aided by a similar publication on the part of the great hospital establishments in civil life; marking not only the effects of medicines, but the changes that may have taken place in the general character and constitution of the patients on whom they have been tried, and in the symptoms of the diseases with which they may have been affected. But to return to the work before us.

3. Cold affusion. This remedy, Mr B. thinks, is much too sparingly used in the fevers of India; he even conceives that it never has had a fair trial. Reasoning *a priori*, we should be inclined to suppose it a most valuable addition to our curative means in a climate such as India; and in the practice of our author it seems to have proved so, particularly in the early stages. We can readily conceive, however, that the great difficulties and inconveniences of using the cold affusion in a number of patients, in a large hospital establishment, must have occasioned that general neglect of it which Mr Ballingall mentions The ablution or sponging, we can well suppose, would be the more favourite mode of employing the agency of cold

4 Mercury. We now come to the Herculean remedy of the east; and as Mr Ballingall has evidently founded his opinions, with regard to its powers, on his own practice, uninfluenced by authority, we shall transcribe his observations entire.

" *Mercury.*—In the common fevers of the Madras establishment, this is a remedy which, by a liberal use of the evacuations already recommended, may very generally be rendered unnecessary. Where, however, the patient has laboured under an indifferent state of health previous to the accession of urgent febrile symptoms, and where the use of purgatives alone seemed inadequate to the restoration of healthy secretions, and natural alvine evacuations, I have frequently had recourse to it with the very best effects; and, in the more severe forms of fever prevalent in India, it is a remedy reckoned indispensable. While some difference of opinion exists regarding the mode of its operation, there seems to be but one opinion generally prevalent regarding its extensive utility. On this subject I speak with diffidence, but am inclined to think, that a more free use of bleeding and purgatives would, in a great many cases, even of these severe fevers, supersede its necessity. When, from the neglect, or too limited use of early evacuations, or from the unusual inveteracy of the disease, we find that it has terminated in a very imperfect cure, and that the patient remains subject to frequent relapses, often assuming an intermittent form, we have generally reason to suspect that the disease has fixed upon, and impaired the action of the liver. This is the more to be apprehended, if a variable and uncertain appetite, an irregular state of the alvine evacuation, a foul tongue, a pearly eye, sometimes tinged with yellow, a sallow complexion, and a parched skin, give us additional reason to suspect a fault in this viscus; and although no pain in the region of the liver should guide us to the seat of the disease, we are to proceed as if it were the organ principally affected. In the circumstances above enumerated, a careful examination of the abdominal parietes will sometimes detect an enlargement of the spleen. This, however, we have reason to believe, may exist to a considerable extent, and for a length of time, without any serious injury to the general health; and it becomes chiefly of importance, by giving us additional grounds to suspect a lesion of the functions of other organs, which we are led to consider as more important, perhaps only because they appear less obscure. Mercury in this state of things is a remedy in which I have been led to place the most unbounded confidence; and although, perhaps, often of itself adequate to the restoration of health, should always, wherever the patient's circumstances will permit, be assisted by change of climate, and a temporary absence from the station where the disease has been contracted. Long continued, or even repeated courses of ptyalism, succeeded by a journey to the sea-coast, or a trip to sea, will frequently succeed in restoring the patient to a competent share of health, and enabling him to resume his duties in India; and, should these means fail, we have then no resource left but to recommend a return to Europe." pp. 34—36.

In the employment of Emetics our author has been very guarded, and has principally used them where a previous accumulation of bile in the stomach, or the presence of some recently swallowed substance, had occasioned the rejection of purgatives.

5. Bark. The propriety of the employment of this far famed drug seems to us next to bloodletting to demand an investigation The excellent surgeon who first brought its powers in gangrene into question among British practitioners, (Sharp, Critical Inquiry,) has employed a language which, with little variation, might, we think, be equally applicable to its administration in many other diseases. " In certain cases, to distinguish how far the remedy, and how far nature operates, is probably beyond our discernment." According to Mr B, its utility in continued fever is more than doubtful; even in intermittent he has not found its exhibition attended with that decided success which he had been led to expect; and in numerous instances the disease was removed by arsenic after the bark had

failed. Of its utility in India we cannot judge; but in the continued fevers of this country, we believe it might with great safety be erased from the list of remedies altogether.

Our bounds will not permit us to follow Mr Ballingall minutely into the history of Dysentery and Hepatitis, which occupy the rest of his book. The account of the dissections of the former disease, in our opinion, deserve great praise. From them it appears, that, in 10 cases out of 35, the liver was found free from disease, and the morbid appearances were chiefly confined to the larger intestines. Two portions of the gut were found peculiarly liable to the disease, viz. the cœcum, with its vermiform appendix, and the sigmoid flexure of the colon. This Mr B. accounts for from the dependent situation of these two portions of the canal.

> "When we are in an erect posture," he remarks, "the cœcum occupies a very dependent situation, with respect to the origin of the superior mesenteric artery from which it is supplied. The same thing cannot be said of the sigmoid flexure of the colon, so long as we continue in this posture; for it is then as high as the origin of the inferior mesenteric which supplies it: but, when the patient comes to be bedridden, and lies alternately on the right and left side, these two portions of the intestine come alternately to occupy a situation very favourable to any accumulation of blood that may be disposed to take place. And the position of the part which has such an obvious influence in other cases, will probably be allowed to operate with more force here, when we consider that the veins of the abdominal viscera are destitute of valves, and that although, upon the whole, the blood is assisted in its passage through them by muscular pressure, yet it is not, as in the veins of the extremities, assisted in its progress through each individual trunk by the action of circumjacent muscles." p. 55.

From all the appearances of this disease, both in the living and dead subject, Mr B. conceives that two varieties are very distinctly proved to exist in India. The one an acute disease, confined chiefly to the large intestines, the "colonitis" of the Indian practitioners; the other a more chronic form of the disease, and more extended in its site, "the hepatic flux."*

We shall briefly state his remarks upon the different remedies employed, premising, that, like the great majority of practical authors of the present day, he denies the contagious nature of the disease.

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* So far back as the days of Paulus Ægineta, (De re Medica, lib. iii. cap. 42,) the distinction of hepatic dysentery had been made, and the similarity of the discharges to the washings of raw meat had been observed. Trallian also (Cap. 9. De Intestinorum Ulceratione quæ proprie Dysenteria Græcis dicitur) mentions those dysenteries in which the intestines were ulcerated, but where the liver and other viscera were unaffected.

Bloodletting.—From the days of that eminent military surgeon, Leonardus Botallus, the employment of this remedy has been frequently revived in dysentery. Aetius (Tetrabib. serm. 1. cap. 43, et seq.) had, indeed, recommended it, particularly in plethoric habits, where the strength permitted, and where any accustomed discharge of blood was suppressed; and Alexander Trallianus (Lib. 8, cap. 8, De Dysenteria quæ Rheumatica dicitur) had advised general bloodletting to an extent of two *heminas*, or little less than twenty ounces; but to Botallus is certainly due the credit of re-establishing the practice. In our own times, and indeed within the period of the last three years, a tract has appeared from the pen of Dr Somers, late physician in chief to the army in the Peninsula, in which the great advantages to be derived from venesection are clearly pointed out; and the value of the practice receives still further corroboration from the work before us. We shall not prosecute any further the parallel between the opinions and the practices of Mr Ballingall and those of other authors, whether modern or ancient, because it appears very obvious to us, that that gentleman has purposely adopted the more useful, though less showy, plan of describing the appearances which he met with himself, and stating the effects of remedies as observed on his own patients, without any reference to the descriptions or practice of others; hence the following remark on bloodletting in dysentery must carry with it a considerable share of importance. "In the few cases of dysentery in which I have employed bleeding, the majority have, I think, terminated favourably; and of those in which the result has been fatal, the appearances on dissection have been such as to excite a sentiment of regret at not having carried the evacuation farther." p. 68. Of topical bleeding Mr B. appears to have had more experience, and he gives a very interesting extract from a letter on the subject from the pen of Dr Annesley. He also is disposed to think favourably of the common French practice of leeches to the anus.

Purgatives he approves of so long as unhealthy feces are carried off by their use; but when this is not the case, and when they only aggravate the pain and tenesmus, and increase the bloody and mucous discharges, they become unnecessary at best, but often injurious,—an opinion in which we most fully coincide. He has chiefly employed the neutral salts, with or without infusion of senna, from a conviction that they operated more effectually, and with less griping and irritation, than other remedies of this class.

Of Emetics he has had no experience in either form of dysentery.

Sudorifics he has used to a large extent. A modification of the compound of opium and ipecacuanha introduced by Mr Abercromby of the 34th regiment, viz. first giving several grains of the solid opium, and then following it by the exhibition of two or three ounces of an infusion of ipecacuanha, he considers as acting principally as a sudorific; but the pill of opium, combined with antimonial powder, he has found a more convenient form, particularly in camps. He cautions us, however, that smaller doses of this, as well as of other sudorifics, are required in India than in colder climates.

To the Warm Bath Mr Ballingall attributes all the virtues usually assigned to or expected from it in European practice; but, while treating of its diuretic effects, he makes the important practical remark, that the affection which peculiarly seems to demand its employment with this intention, consists rather in a real suppression than in a retention of urine.

Of the uses of Blisters, Injections, and Mercury, he speaks in succession, and to the two first he attributes a considerable share of importance as adjuvants. His observations on the employment of the latter remedy are few; but they are important, and appear to us to be brought forward with great impartiality, and obviously without any attachment to a particular theory, but solely from the result of considerable practice. Our limits will not allow us to accompany him fully; but we may observe, that, while he deprecates the use of mercury in the pure inflammatory dysentery, and even thinks that it often produces the very symptoms for the removal of which it is administered; in the chronic form he recommends an " implicit reliance on it." To the mode of administration he does not seem to attach much importance, but, upon the whole, he inclines to the exhibition of the blue pill.

We shall abstain from any remarks upon his last division, in which Mr Ballingall treats on Liver Complaints. His observations are few, but he announces his intention of extending them. What he advances in his present work, appears to have been suggested solely from practice, and indeed throughout his book, he seems faithfully to have followed nature; a species of writing of which we entirely approve. A medical writer cannot check too rigidly the effervescences of fancy, and the flights of imagination; in a poet, indeed, whose mind, as well as his eye, is often " in a fine frenzy rolling," excellence, in a great measure, depends upon the exercise of these gifts. The poet, too, it is agreed, succeeds best in fiction; the medical, like the legal writer, derives his value from being strictly a matter of fact man. Long habits of contemplating disease, and rigid fidelity in reporting upon it, are the leading requisites in a medical author. It is to this, more than to their learning, or the fascination of their style, that the works of Pringle, Lind, and Blane, owe that celebrity which will never decay. This it is which gives to the more modern writings of many of our naval and military brethren an expression, which renders their reports on the diseases of foreign climes so impressive, and so valuable. Varieties of style, peculiar felicities of language, or apt application of literary lore, may designate the different species of authors; but it is the happy combination of experience, judgment, and candour, which marks the individuals deserving of our confidence.

The medical profession, perhaps, more than any other, teems with adventurers who offer their crudeand baseless speculations as the results of profound philosophical investigation. Some there are, it is true, who, having long guided the "sheers of Clotho," and seen countless patients, but little of disease, dignify their descriptions of daily routine with the title of practical experience. Their works too often display the practice of the trade, not the profession; and the spirit of the craft, not the philosophy of physic. A much more numerous, and an infinitely more dangerous class, are those who, having seen scarce any thing of disease, except some prominent symptoms,—by the help of wild physiological notions, and new modelled pathological opinions, found theories which *they* deem philosophic; a few examinations of the dead, conducted, it is to be feared, in a very cursory manner, are produced to confirm their opinions; on this flimsy foundation they compose volumes, which they offer to the profession as the results of *their extensive experience.* The claims of these sciolists to experience in physic, irresistibly call to our mind those of Smollet's medical beau to knowledge of the world; they were founded upon his having cruised "for three whole months in the channel," and we are as little disposed to place our professional faith in the one, as the novelist's hero was to confide the control of his pecuniary concerns to the other. The author before us belongs to neither of these classes; his descriptions appear faithful, his theories are conformable to sound physiology; and his examinations of the dead have been numerous, and evidently conducted with care and discrimination. Above all, his practice is simple, judicious, and decided, and is detailed in a plain modest style, altogether free from bombast and affectation.

III.

Lectures on Physiology, Zoology, and the Natural History of Man, delivered at the Royal College of Surgeons. By WILLIAM LAWRENCE, F. R. S. Professor of Anatomy and Surgery to the College, &c. 8vo. pp. 579. London, 1819.

MR LAWRENCE is well known to the public as a man of acknowledged talents and attainments. His high professional character, and the important situation which he holds as Professor of Surgery to the Royal College of Surgeons of London, will procure for his work an extensive circulation among that interesting class for which it is chiefly intended, the younger part of the medical profession; and it is, therefore, with deep regret that we find in it certain opinions from which we are under the necessity of entering our strong and decided dissent. To this part of his work we mean to confine our observations; and, with all the respect that we feel for eminent talents, we reserve to ourselves the right of examining these opinions, and of stating, in the plainest terms, the reasons which induce us to believe that they are unphilosophical in their nature, and dangerous in their tendency.

The nature of these doctrines, and the mode of reasoning by which they are supported, will be illustrated by the following extracts:

> "The same kinds of facts," says Mr Lawrence, (at p. 104,) "the same reasoning, the same sort of evidence altogether, which shew digestion to be the function of the alimentary canal; motion of the muscles, the various secretions of the respective glands, prove that sensation, perception, memory, judgment, thought,—in a word, all the manifestations called mental or intellectual,—are the animal functions of their appropriate apparatus, the central organ of the nervous system."
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> "Shall I be told, that thought is inconsistent with matter; that we cannot conceive how medullary substance can perceive, remember, judge, reason? I acknowledge, that we are entirely ignorant how the parts of the brain accomplish these purposes, as we are how the liver secretes bile, how the muscles contract, or how any other living purpose is effected,—as we are how heavy bodies are attracted to the earth, how iron is drawn to the magnet, or how two salts decompose each other." "In opposition to these views, it has been contended, that thought is not an act of the brain, but of an immaterial substance residing in or connected with it." By this doctrine, Mr L.

conceives "the brain is left almost without an office. It has, indeed, the easiest lot in the animal economy; it is better fed, clothed, and lodged than any other part, and has less to do. But its office, only one remove above a sinecure, is not a very honourable one; it is a kind of porter entrusted to open the door, and introduce new comers to the master of the house, who takes upon himself the entire charge of receiving, entertaining, and employing them." The same opinions are repeated under various forms of expression. "No feelings, no thought, no intellectual operation, has ever been seen, except in conjunction with a brain." And, recurring to the favourite illustration of the liver, it is asked, "What should we think of a person who told us, that organs have nothing to do with the business; that cholera, jaundice, hepatitis, are diseases of an *immaterial hepatic being?*"

We cannot extend our extracts much farther, but the following passage we must give at length.

"Sir Everard Home, with the assistance of Mr Bauer and his microscope, has shewn us a man eight days old from the time of conception; about as broad, and a little longer, than a pin's head. He satisfied himself that the brain of this homunculus was discernible. Could the immaterial mind have been connected with it at this time; or was the tenement too small even for so etherial a lodger? At the full period of utero-gestation, it is still difficult to trace any vestiges of the mind; and the believers in its separate existence have left us quite in the dark on the precise time at which the spiritual guest arrives in his corporeal dwelling—the interesting and important moment of amalgamation or combination of the earthy dust and the etherial essence. The Roman Catholic Church has cut the knot which no one else could untie; and has decided, that the little mortal, on its passage into this world of trouble, has a soul to be saved; it accordingly directs and authorizes midwives, in cases of difficult labour, where the death of the infant is apprehended, to baptize it by means of a syringe introduced into the vagina, and thus to save it from perdition." p. 107.

These extracts will enable our readers to form some idea of the light and easy and summary manner in which Mr Lawrence disposes of one of the most serious subjects that can occupy the attention of a reasonable being. When the introductory lectures from which the extracts are taken were formerly published by themselves, these opinions were considered by many as amounting to materialism, and consequently as being highly dangerous to the young men attending the lectures instituted by the Royal College of Surgeons. In this light they were viewed by Mr Abernethy in his "Physiological Lectures;" and in the complete edition of Mr Lawrence's Lectures now before us, we have a section entitled, "Reply to the Charges of

Mr Abernethy." This reply consists of little more than repeating the same opinions, and arguing in defence of them by confounding the important distinction between an immaterial soul, and the doctrine of a subtle essence, resembling electricity or magnetism, which some have conceived to be the principle of organic life, and the cause of the functions of a living animal.

> " A party of modern sceptics!—A sceptic is one who doubts; and, if this party includes those who doubt, or rather who do not doubt at all, about the electro-chemical doctrine of life, I can have no objection to belong to so numerous and respectable a body."—" Examine the mind, the great prerogative of man. Where is the mind of the fœtus? Where that of the child just born? Do we not see it actually *built up before our eyes* by the actions of the five external senses, and of the gradually developed internal faculties?"—" Where, then, shall we find proofs of the mind's independence on the bodily structure? of that mind, which, like the corporeal frame, is infantile in the child, manly in the adult, sick and debilitated in disease, phrensied or melancholy in the madman, enfeebled in the decline of life, doting in decrepitude, and *annihilated by death?*"

This, it is obvious, is only a repetition of the obnoxious opinions which this introductory section is meant to defend, and now comes the defence. Be it known to our readers, that, in the very strong passages which we have quoted from Mr Lawrence, and many others of a similar import, he did not mean to allude at all to the separate existence of the soul. Mr Lawrence only " *speaks physiologically*."—" This sublime dogma" (the separate existence of the soul) " could never have been brought to light by the labours of the anatomist and physiologist," but " rests upon a species of proof altogether different."—" An immaterial and spiritual being could not have been discovered amid the blood and filth of the dissecting-room." Now, that a doctrine should be really true, and physiologically false, and the object of ridicule, is a contradiction in terms. It is, besides, nothing to the purpose; for whether Mr Lawrence was speaking physiologically, or theologically, or metaphysically, he has maintained, in the plainest terms, that thought is a function of matter; and if we are to use words in their ordinary acceptation, this is the sum and substance of materialism. But there is another point of view in which we think this reply quite unworthy of Mr Lawrence; for what does it amount to? First, Mr L. maintains, in the most unqualified terms, that matter may think, and that the substance of the brain actually does think. In support of this doctrine various arguments are brought forward, and all his powers are exerted to turn into ridicule the opposite doctrine of an immaterial soul. All this was impressed upon the young men attending his lectures, with great variety of illustration, and without any explanation or qualification whatever. But when Mr Abernethy, a man of high talents, publicly charges him with the obvious tendency of these opinions, then he shrinks from them;—he meant no such thing,—nothing against the immateriality of the soul;—he was only speaking physiologically; – he only intended to say that this immaterial being is not discovered amid the blood and filth of the dissecting room; or, in plain English, that, by looking at the brain of a dead man, no one could learn that his soul is immortal! This is certainly true; but if this be all that Mr Lawrence meant, we do not know with whom he is contending. We never heard of any anatomist who pretended to exhibit in his dissecting-room an immaterial soul; and we would ask Mr Lawrence, Has he been more fortunate in his dissections in support of his own hypothesis?—Has he shewn his pupils a brain in the act of thinking;—or has he, even "with the assistance of Mr Bauer and his microscope," shewn them a single thought that had been recently produced from it? Something of this kind is necessary before we can admit that "the same kind of evidence that shews digestion to be the function of the alimentary canal, motion of the muscles, and the various secretions of the respective glands, proves that thought is the function of the brain." We lay open in the dead body the alimentary canal, and we trace the articles of food through the various steps of the process of digestion. We examine the various glands, and we find in the ducts connected with each of them a portion of that fluid which is its appropriate secretion. The contraction which constitutes motion we see in the living body when muscles are exposed during surgical operations, and in a body recently dead we can produce it by the application of galvanism and other stimuli. That digestion, therefore, is the function of the alimentary canal, motion of the muscles, and the various secretions of the respective glands, we have the most direct and satisfactory evidence. Have we any such evidence that thought is the function of the brain? Has a single thought recently secreted ever been discovered in the dissecting-room of Mr Lawrence? If not, this part of his argument refutes itself; and, by his own shewing, he has left this important question just as he found it.

This specimen of Mr Lawrence's mode of reasoning will shew that it is not very profound; equally flimsy are the other parts of it, which, in our opinion, contain not the shadow of argument. What avails it to say, that we never saw thought without a brain? Did Mr Lawrence ever see it without a heart, or lungs, or a liver? What avails it to say, that, though it cannot be explained how the brain thinks, no more can it be explained how the liver secretes bile, or a muscle moves, or a stone falls to the ground. This is true, but has nothing to do with the question; it is not the explanation that we want, but the fact That the liver secretes bile, that muscles move, and that stones fall to the ground, however inexplicable to us, are undoubted facts; that the brain thinks, is not a fact, it is only the opinion of Mr Lawrence. But, farther, says Mr L. upon the supposition of an immaterial part, the brain is left without an office. This is new to us. The brain is the centre of sensation and motion; and every organ of the body depends for its healthy action upon an influence derived from it. Cut off this influence from the eye and it sees not; from the ear and it hears not; from the hand and it neither feels nor moves; from the heart and it ceases to beat; from the organs of breathing and respiration is at an end; even the glands cannot perform their secretions aright without this influence. Such and so extensive are the offices of the brain; if we may borrow the figurative language of Mr L., though it be well fed, clothed, and lodged, it has quite enough of work upon its hands, without assigning to it the additional labour of thinking. Again, alluding to the zoophytes, who live in both halves after they have been divided, Mr L. asks, is the immaterial principle divided? Certainly, if there be such a principle, it is no longer immaterial, for it is divided, and whatever is divisible is matter. But did Mr L. ever hear of a man being divided into two, and both halves continuing to think? The subject is too absurd to enlarge upon; and we merely mention it here as another example of Mr L.'s mode of reasoning, and one to which he repeatedly recurs, applying his argument to mere organic life, and then extending it, as if by necessary consequence, to the functions of mind, to which it has no relation whatever.

The observations which we have now made embrace the leading points in Mr L.'s argument, if argument it can be called. But, as the subject has lately excited much attention, we avail ourselves of this opportunity of delivering our opinion more fully upon the general question of materialism, particularly in the form which it has lately assumed, as connected with certain speculations on vitality and organization. If, in the course of this investigation, we shall refer to topics not properly connected with a physiological discussion, let us not be misrepresented. Let us not be charged with dragging in religion to decide a physiological controversy. The blame rests with those who have perverted physiology to the support of scepticism.

We know nothing of the essence or occult qualities either of matter or of mind. We see around us in the world, and in the structure of our own bodies, various substances which are solid, extended, and divisible, and to this particular combination of properties we give the name of *matter.* We are conscious of a power within us, which thinks and reasons, desires and loves, and to this particular combination of properties we give the name of *mind.* The words Matter and Mind are mere arbitrary terms, intended to express these two combinations of properties, which are quite distinct from each other. Of the various substances to which we give the name of matter, the properties are obvious to our senses, but we have not the smallest reason to suppose that any one of them thinks. Of the power which we call mind, the properties are known to us only by consciousness; they are obvious to none of the senses; and we have not the slightest reason to suppose, that it is either solid, extended, or divisible. It has nothing in common with that which we have called matter, we therefore say it is *not matter*, or, in other words, it is *immaterial.* Now the name is nothing; when we say it is immaterial, we merely say it is not matter: and when we say it is not matter, we merely say it is not solid, extended, or divisible: it has properties essentially different from that class of substances to which we have given the name of matter, and has nothing in common with them. If any one say that matter may think, it is not properly to be treated as unsound reasoning: it is a mere absurdity or contradiction in terms: for matter is only a name which we have given to something which is solid, extended, and divisible; and thinking is the property of another something which we know only by that property, and which we have not the slightest reason to suppose is either solid, extended, or divisible. To say, then, that matter thinks, is to say, that which is solid, extended, and divisible, thinks; but that which thinks is not solid, extended, and divisible: therefore to say matter thinks, is to say it is matter and it is not matter. But the existence of a function implies the existence of something to which the function belongs; thinking therefore is a function of something; it is not a function of matter, for it has nothing in common with it: therefore the being that thinks is *not matter*, it is *immaterial.*

These fundamental principles we hold to be the dictates of common sense, and upon every principle of sound reasoning incontrovertible. But there are other views of the subject which lead to the same result. If thinking be a property of matter, it must either be an essential property of it, or a power superadded to it, in that state which we call living or organised matter. The essential properties of matter are necessarily inherent in, and inseparable from, the whole mass, and every particular part into which the mass may be divided. If thinking then be an essential property of any particular mass of matter, it must also be a property of all the parts of that mass when it is divided. But this is contrary to fact,—therefore thinking is not an essential property of matter. Is it then a property superadded to organised matter, or, in other words, is it a function of the brain?

Many remarkable properties or functions are superadded to organised matter, to which of these has thought any affinity? It is not digestion; it is not reproduction: it must either be sensation, secretion, or motion.

(1.) Is it sensation? The senses are very remarkable properties of organised bodies; but they are merely passive. They are the properties by which the body receives impressions from external material objects, and without these impressions from matter, they are as nothing. The eye may be sound, but without the presence of a luminous body, we see not; the ear may be acute, but without a sounding body, we hear not. Besides this immediate dependence upon impressions derived from matter, the senses are limited in their operation by precise material boundaries:—we see not beyond a certain distance; we hear not beyond a certain range; we feel not, but from the contact of a tangible body. But is thought a mere sensation?—No! It is something active. Has it any such relation to matter? is it thus confined within material boundaries? No, it is an active, restless principle, which ranges uncontrolled from world to world, from sun to sun, from system to system; in an instant it pervades the regions of boundless space; "how fleet is the glance of the mind." It owns no dependence upon material impressions. In the fairest scene of poetic stillness, the tempest may rage within, and mid the convulsions of matter, the mind may be serenity and peace. It is equally independent of the state of the corporeal functions. While these are the prey of the most frightful diseases, the mind may emanate tranquillity and hope; and when every function is pursuing its course with such placidity, as when an infant sleeps, the mind may be tossed by contending passions, distracted by anxiety, or racked by anguish, remorse, despair. Thought has no affinity to sensation.

Then (2.) Is it secretion? By secretion fluids of various kinds are separated from the general mass of the animal body, and the substances which are separated in this manner, have not only the general properties of matter, solidity, * extension, and divisi-

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* It is almost unnecessary to mention that the term *solidity* is not here employed in that sense in which it is opposed to *fluidity*, but as expressing a pro-

bility, but, by chemical analysis, they are resolved into the same elements as the body from which they are separated. It is indeed the philosophy of common sense, that matter can produce nothing but matter; that whatever is separated from an animal body must have some of the properties of animal substances. But what are the properties of the secretion of the brain? Has Sir Humphry Davy analysed a parliamentary harangue, or a lecture on physiology? have his most delicate tests made any impression upon the Iliad or the Novum Organum? has he even succeeded in decomposing Marmion or the Tales of my Landlord? has he detected the slightest affinity betwixt the substance of the brain, and these remarkable secretions? No, though Mr Lawrence thinks it very analogous to the secretion of bile, thought is certainly not a secretion. Then (3.) Is it motion? "is to shake the same as to think?" No, it is not motion.—Then what is it? It is not a property of matter, and it is not a function of matter in any conceivable form; then it is not matter, it is immaterial.

In answer to this reasoning, does any one maintain, though it is neither sensation, secretion, nor motion, it is a different function altogether, to which nothing analogous exists in the other functions of an animal body? We repel the position as perfectly inadmissible; it is a mere *petitio principii*, a taking for granted of the thing to be proved. We have no conception of organised matter, exercising any other functions than those we have mentioned, motion, sensation, and production; and thought has not the slightest affinity to any of them, therefore it is not a material function.

There are various other views which might be taken of this important ubject and which we think must satisfy every candid inquirer, that the phenomena of mind are utterly inconsistent with any idea we can form of a material function. Let us take a single example, and inquire in a few words, how, upon the principles of materialism, we are to account for the phenomena of memory? We find some difficulty in conceiving how the matter of the brain receives the impressions of history, philosophy, and mathematics; how various sciences and various languages, with the addition of innumerable affairs of ordinary life, are all received without confusion into "one small head." But we think there is still a greater difficulty, and that is, the manner in which they are retained. It is well known to physiologists, that all the parts of a healthy body are in a constant

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perty which is common to all material substances, whether solid, fluid, or gaseous, the property of having a real substance filling a certain determinate space.

state of change; that there is a constant, though gradual removal of the old matter, and a corresponding deposition of new matter in its place. We cannot ascertain the precise periods connected with this remarkable process, but we have every reason to believe that there is a period, and not a very long one, during which every particle of the body is renewed. Now, if memory be merely an impression made upon a material organ, in what manner is the impression transmitted? Does an old man relate the tales of his early days? How has he retained them? There is not in his body a single particle that was present when the incident occurred, upon which he dwells with such complacent garrulity. Has one series of particles, as they departed, related the tale to those who came to occupy their place, as a centinel, on quitting his post, repeats his instructions to him who relieves him? Or is there some provision "to the world a secret yet," by which the newly acquired matter is instructed in what it is expected to know? Does the new matter that is added to the body of a Gregory, undergo from time to time a course of instruction in physic? Was the nutriment of Porson regularly drilled in Greek? Do the particles of Wilberforce relate to one another the tale of Afric's wrongs? Did the atoms of Bonaparte urge on the conscript atoms in their wild career of ruthless ambition? Is it to this simple and easy course of transmission, that we are all indebted for what little learning we have been able to retain; and have certain negligent atoms failed in their duty in regard to much useful knowledge that we all have forgotten? We cannot enlarge upon this curious subject; but it is highly important in a moral point of view, especially in regard to crimes and punishments. Upon the principle of materialism, it will evidently be necessary, that the punishment shall immediately follow the offence, for, if a single day intervene betwixt them, some of the peccant matter will have eluded our grasp, and some innocent matter will be punished with the guilty. Upon this obvious principle, what shall we say of a remarkable instance, in which an individual suffered the highest award of British justice on account of a crime committed in a foreign country twenty years before? Whatever ideas we may entertain of the enormity of the offence, we shall be obliged to acknowledge, that, long before the period we have mentioned, every particle of the real offender had "absconded from justice."

But why talk we of morals? Upon the principles of materialism there can be neither morals nor moral responsibility. Are "all the manifestations which we call mental or intellectual the functions of the brain, as digestion is of the alimentary canal, motion of the muscles, and the various secretions of the respective glands?"—Then justice, humanity, benevolence, and patriotism, are the healthy condition of this important function; and injustice, robbery, murder, and sedition, are merely the function in a state of disease. But why should diseases of this function be considered as objects of punishment, while hepatitis and jaundice escape with impunity? "'Tis strange, 'tis passing strange," that, in this enlightened nation, one function should be left to the care of the physician, while another is consigned to his majesty's advocate for his majesty's interest; that calomel and Cheltenham water should be considered as remedies adapted to the one, while, for the other, nothing will answer but Newgate and Tyburn-tree!

But enough of this. Materialism is formidable only when viewed at a distance: when closely examined, it is a system from which the mind revolts as perfectly inconceivable. They who require us to believe that the intellectual manifestations of a Newton were a mere corporeal function, resembling the secretion of bile, require us to stretch our faith, till faith utterly fails us. From such a monstrous dogma the mind willingly takes refuge in the belief of an immaterial being The only question then is, Whether the existence of an immaterial being be really probable? and who can doubt that it is not only probable, but certain and necessary? The whole phenomena of nature compel us to believe the existence of a great First Cause. This cause must be a living agent; and this agent must, of necessity, be independent of material organization;—in other words, he must be immaterial. The existence of such an essence as an immaterial being is, therefore, not only probable but necessary; and that this Being, the great cause of all things, should create immaterial beings, is fully as probable as that he should create material beings. Without pushing this argument farther, then, there is no improbability in the belief of an immaterial soul.

Now, if thought be not a function of matter, we have no reason to suppose that it should be affected by any change in the combinations of matter. What is death? It is merely a change of combination. That which was formerly a living body is then resolved into oxygen, hydrogen, carbon, and azote, and various new combinations of these elements. If thought, as we have seen, has no affinity to matter, we have no reason to suppose that it should be affected by such a change of combination. But thought is something active, and must, therefore, be the function of an active living being It is, therefore, in the highest degree probable, that the being which thinks shall continue to think after that change which we call death; and if it continue to exist and to think after that change, have we any reason to believe that it shall at any time cease to exist? We never knew any thing cease to exist. After the change which we call the death of an animal body, it can be demonstrated, upon the strictest principles of chemistry, that not a particle of that body ceases to exist. We have no conception of annihilation; our whole experience is against such an idea. Now, the being which thinks and reasons has a real existence, independent of any combination of matter. We have no reason to believe that it is affected by any change in the combinations of matter, or that it is itself resolvable into elements; have we then any reason to conceive that it should ever cease to exist?—None whatever. It is therefore in the highest degree probable that it is immortal. *

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* We are aware of an objection that may be urged against some points in this argument,—that it leads to the belief of an immaterial and immortal principle in brutes, or in the words of a late writer, " it immortalizes the dog that barks at us, and the insect that annoys us in our sleep."—Now, though this were really a fair inference from the argument, it would neither destroy nor weaken it; it would have nothing in it of the nature of a *reductio ad absurdum*, but would leave the great question of the immateriality of the human soul quite unaffected. But is it really such a consequence of the argument? We conceive that it has nothing to do with it. We have already alluded to the important distinction betwixt an immaterial soul, and the mere condition of organic life. The latter we have not the slightest reason to consider as immaterial, but merely as a property superadded to matter in a particular state of combination, for in the zoophytes it is divided. The objection therefore can only apply to the higher classes of animals, in whom we perceive something resembling the manifestations of mind. Now, our knowledge of mind is derived entirely from consciousness. We observe numerous bodily actions which in ourselves result from mental operations, or, in other words, are manifestations of mind; but the question comes to be, do these necessarily imply mental operations?—What do we see in the automaton?—It moves, it writes, it draws figures, it performs music, it plays at chess. These actions are in ourselves manifestations of mind, but here they are the result of mere mechanism, set in motion by an impulse communicated by the exhibitor. Why may we not suppose that certain manifestations in animals may result from particular arrangements of matter, ordained by the Creator to act in a certain manner, and to be set in motion at certain times by bodily feelings or by an impulse communicated through the senses. They resemble the manifestations of mind, but they are clearly distinguished from them. We call them instinct; and even in the highest degrees of it we perceive the line drawn with great precision betwixt it and reason.

The acts of animals that are regulated by instinct, we have every reason to believe, are called into action by bodily feelings, or by impressions on the senses, and they are regulated by fixed and determinate laws. This uniformity and regularity shews something remarkably different from those actions in man which are governed by reason. These are called into action, not by impulse, but by motives, and motives are addressed to mind. Hence a power of reasoning and deliberating; of acting or abstaining from action; of following a present impulse or of resisting it, under the influence of a motive which refers to some-

We are led to this conclusion by every idea we can form of matter and of mind: and we are confirmed in it by every view we can take of the operations of mind itself, and of the order and harmony of the universe. The whole phenomena of nature compel us to believe the existence of a great First Cause, and every department of it exhibits ample evidence of wise design and boundless power. From the planets that revolve in their appointed orbits, to the insect on which we tread, each is endowed with properties exactly suited to the situation in which it is placed, and each fulfils in the most perfect manner the purpose for which it was created. In this fair field of harmony and order, the only anomaly is man. He alone is endowed with powers, for which there appears no adequate range, in the spot of earth on which he is fixed, and the point of time to which his existence is limited. After a long and helpless infancy, and a protracted period of intellectual culture, his powers at length expand in all their glory, and oft, alas how oft! when they have just opened in their meridian splendour, is he cut down as a flower and withereth? If this be his end, he is an anomaly which in the wide expanse of creation stands alone; a beacon set to record the fact, that, in the mightiest works of omnipotence, something has been made in vain. But farther, can the being who has introduced such order and harmony into the whole economy and course of nature, be indifferent to the disorder that pervades the moral world; to virtue writhing beneath the iron hand of oppression; to vice triumphant, raising its head and defying the face of heaven; to deeds of foulest aspect, unseen by mortal eye; to crimes of the blackest malignity, of which no human law takes any cognisance, and no human power requireth vengeance? No,—it cannot be. There must be a harmony yet to be disclosed; there is,—there must be,—something immortal. But in this great inquiry, shall we allow nothing to the operations of Mind itself, to the power, that we feel within,

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thing future, and which consequently can exist only in mind. This power of being influenced by the future in opposition to present feelings and present impulse, indicates at first sight the remarkable distinction betwixt reason and instinct; betwixt man and animals. We see its influence in man in the daily occurrences of life, and it leads us to the high principle of moral responsibility, to which animals exhibit nothing in the smallest degree analogous. There may be in animals much appearance of contrivance and sagacity; but wherever there is this acting upon impulse, and this inability to resist impulse under the influence of something future, there is nothing of the nature of mind or soul, nothing of the nature of moral responsibility, and nothing that we have the slightest reason to believe is either immaterial or immortal. On this point and several others connected with this subject, see Drew on the Immateriality of the Soul.

which revolts from the thought of ceasing to be? Whence the sublime conception of surviving the wreck of matter? Whence the might that animates the good man, while, with every fibre quivering in death, he exults in the prospect of immortality? Whence the arrow that rankles in the breast, when the stoutest mind has been wounded by deep remorse? Whence the pang, unseen by human eye, and unknown to human ken, that can chill the heart which never feared before, and make it shrink with all the terror of childhood from the stillness of the tomb? Can this be a property or a function of matter?—No, it cannot be. They have reasoned well who have told us, that, in all these workings of a mighty mind, there is,—there must be,—something immortal.

Before we leave this subject, there is still another view of it, on which we wish to make a few observations. We set aside the principles which we have now referred to, and we admit, for the sake of argument, that the hypothesis of materialism may be brought out without absurdity and contradiction. We then proceed to try it, as we would try any other hypothesis, by its agreement with the phenomena. In this view we wish to state to the younger part of our readers, in what manner the facts which we learn from physiology and pathology bear upon this most important subject.

An unprejudiced inquirer, investigating the subject upon the mere principles of physiology and pathology, we think would state the question in the following manner. On the nature of thought and reason there are two hypotheses. The one is, that they are directly dependent on organization, being the functions of the brain, as sight is of the eye, and hearing of the ear; that they are impaired when this organization is impaired, and cease when it is destroyed; farther, that the function is built up before our eyes, by the action of the five external senses. This is the hypothesis of Mr Lawrence, and of some philosophers of the French School. The other hypothesis is,—that thought and reason are properties of a distinct immaterial being, which is united to the body of a living man, but may exist after this union is dissolved. This is the hypothesis of Boyle, and Locke, and Newton—of Haller, and Boerhaave, and Dugald Stewart. Now, in regard to these two hypotheses, we are disposed to admit, for the sake of argument, that, upon the mere principles of physiology, we may bring forward the one as well as the other; that physiology does not directly prove either of them; the question therefore is, with which of them do the inductions of physiology and pathology most accord? which of them is most agreeable to the phenomena?

On the very first view of the subject, there are several points connected with the first hypothesis, which we think must shake a philosophic inquirer. Thought and reason are the functions of the brain; but the brain is not sufficient to produce the function, for it requires to be built up by the action of the five external senses. This is rather incomprehensible; but, without stopping to examine it, we cannot fail to remark, that, in many of the inferior animals, the external senses are as perfect as in man: how then does it happen that these have never built up any thing resembling the reasonable human soul? Here is an effect ascribed to a cause, and here is the cause operating in the most perfect manner, in ten thousand times ten thousand instances, without in any one instance producing the effect that is ascribed to it. But farther, is reason a function of the brain? Does this effect then bear no proportion to its cause? Has the function no ratio to the organ on which it is so immediately dependent? On this part of the subject no light is afforded by physiology; for in the inferior animals there is found a brain, possessing the same mechanical and chemical properties as the human brain; and in some of them fully equal to the human brain, both in the relative and absolute dimensions. In relative dimensions the brains of many animals exceed the human. One of the largest is that of the canary bird, which, in proportion to the size of its body, is twice the size of the human brain. And in regard to absolute dimensions, without referring to the elephant, we need only state, that the brain of a seal, six feet long, is fully as large as the brain of a man. In this case again, the cause appears without the effect,---the organ without the function that is ascribed to it. The organ also, be it remembered, is, in all its obvious and acknowledged functions, (those, viz. which relate to sensation and motion,) as perfect as in man, but the rational soul appears not. Does the want of speech obscure the proofs of its existence? No:—for Mr Lawrence has shewn us that in several animals the organs of speech are as perfect as in man, and that they are prevented from speaking only by the absence of reason, not by any defect of organization.

It is mere trifling to allege, on this part of the subject, that the remarkable difference of functions assigned to the human brain, does not depend upon the size of the organ, but on certain peculiarities in its internal structure, or, as it is usually expressed, in the developement of the parts. It is a mere gratuitous assumption, unsupported by the slightest analogy, or rather expressly contradicted by the analogy of all other organs. Betwixt various organs in man and in other animals, and betwixt the same organs in different animals, we in many instances find the most remarkable differences in structure. These differences are admirably adapted to the circumstances of particular animals, but the function to be performed is ultimately the same. Such diversities we observe in the lungs, but all the varieties perform simply respiration:—in the heart and great vessels, but all perform circulation:—in the stomach and alimentary canal, but the most simple perform perfect digestion, and the most complicated do nothing more. Farther, we find no organ that performs a double function; but materialism assigns a double function to the brain. This is evidently unphilosophical; but our limits prevent us from enlarging upon this part of the argument.

We next attend to the light that is furnished by pathology, or the effects of disease on the brain. According to the first hypothesis, reason must be immediately dependent on a healthy state of the brain; must be impaired when it is impaired, and in exact proportion to the extent of the disease; must make progress in decay, as the disease makes progress, and not revive unless the disease be removed. Thus sight depends on a healthy state of the eye. By many diseases which injure the eye, sight is impaired; as the disease advances, sight decays; and there is no example of it being restored, unless the disease which impaired it be removed. According to the second hypothesis, the immaterial soul has an important connection with the brain and the organs of sense, as by means of them it holds intercourse with the external world, and with other living beings. By various diseases of the brain this intercourse is greatly interrupted, or entirely suspended; but the soul itself remains unimpaired.

On comparing these two hypotheses with the phenomena connected with the pathology of the brain, the following inquiries naturally occur to us. In diseases of the brain, do we observe the intellectual faculties decay in exact proportion to the progress of the disease, according to the first hypothesis: or do we, according to the second, see any reason to believe that there exists a soul, which in these diseases is only cut off from its intercourse with us and with the external world? Though it may be thus cut off in a great proportion of instances, do we ever observe this subtile and active being making any attempt to break from the restraint under which it is held? Does it ever remain unimpaired amid extensive destruction of its material residence? Does it ever, after being long obscured, burst out in the midst of frightful ruin?—and thus, though in a few instances, vindicate its claim to independent existence? On this interesting subject, the following observations occur to us, out of many that are recorded by writers of the first authority. A man, mentioned by Dr Ferriar, * who died of an affection of the brain, retained his faculties entire till the instant of his death, which was sudden. On examining his head, the whole right hemisphere of the brain was found destroyed by suppuration. In a similar case by Diemerbroeck, † half a pound of matter was found in the brain; and in one by Heberden, ‡ there was half a pound of water. Mr Marshall mentions a man who died with a pound of water in his brain, after having been long in a state of idiocy. A few hours before his death, he became perfectly rational. § A man, whose case is related by Mr O'Halloran, suffered such an injury of the head, that a large portion of the bone was removed on the right side; and extensive suppuration having taken place, there was discharged at each dressing, through the opening, an immense quantity of matter, mixed with large portions of the substance of the brain. On the 8th day of the disease, Mr O'Halloran remarks, " the sore continued to discharge greatly, insomuch that, when I affirm that three ounces of the brain, with a horrid smell, followed each dressing, I am certain I am a good deal under the quantity." And again, on the 13th day, " the cavern was now terrible, and I feared that the remains of the lobes of the right side of the brain would follow." This man lived to the 17th day. He was paralytic on the left side of his body, but " he retained his intellect to the very moment of dissolution;" and Mr O'Halloran particularly remarks, that, through the whole course of the disease, his mind maintained a remarkable tranquillity. ‖ In a similar case by M. Billot, the patient lived till the 18th day, and retained his faculties until a few hours before his death, when he fell into a kind of stupor. On examining his head, no more than the bulk of an egg was found to remain of the proper substance of the brain. ¶ Besides this extensive disease of the brain in general, it can be shewn, by numerous examples, that the individual parts of it, the pineal gland, the corpora quadrigemina, corpus striatum, &c. may all be diseased or destroyed, without affecting the intellectual powers. Now, if thought be a function of the brain, it must either reside in the whole of it equally, or in some individual part. But it does not reside in the whole; for we have seen in the above examples the

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* Manchester Memoirs, Vol. IV.

† Diemerbroeck, Anatomia, Lib. III. c. x.

‡ Transactions of the College of Physicians, Vol. V.

§ Marshall on the Morbid Anatomy of the Brain, 184.

‖ On Injuries of the Head, p. 103.

¶ Planque, Biblioth. de Medicine, Tom. III.

general mass destroyed to a frightful extent, without impairing thought in any sensible degree. And it does not reside in any of the particular parts which are distinguished from the general mass; for each of these have been destroyed, without affecting it. Therefore it does not reside in the brain.

Is the reasoning faculty, then, thus independent of the most extensive destruction of organization? Does it remain unimpaired amid the most frightful ruin; and, after being long obscured by disease of the organ with which it is most nearly connected, does it, without any change in the disease, burst forth in all its original splendour at the very moment of dissolution? What is the fair, the inevitable conclusion, but that the phenomena of physiology and pathology are directly at variance with the hypothesis of materialism, and in exact accordance with the sublime doctrine of natural and revealed religion—that the mysterious part of our being, which thinks, and wills, and reasons, survives the wreck of its mortal tenement, and aspires to immortality?

Haec studia adolescentiam alunt, senectutem oblectant.—The investigation of the truth on this momentous question is inconceivably important in itself, and it exerts an influence, in the highest degree salutary, on the mind that conducts the investigation in a spirit of grave and chastened inquiry, suited to the serious import of the subject. "Nor is it merely," says Mr Stewart, "with each other that these principles are connected, (the immortality of the soul and natural religion.) They have a relation to all the other principles of moral philosophy, insomuch that a person who entertains just views of the one, never fails to entertain also just views of the other. Perhaps it would not be going too far to assert, that they have a relation to almost all the truths we know, in the moral, the intellectual, and the material world. One thing is certain, that, in proportion as our knowledge extends, our doubts and objections disappear; new light continually breaks in upon us from every quarter, and more of order and system appears in the universe. It is a strong confirmation of these remarks," adds this great philosopher, "that the most important discoveries, both in moral and physical science, have been made by men friendly to the principles of natural religion; and that those writers who have affected to be sceptical on this last subject, have in general been paradoxical and sophistical in their other inquiries. This consideration, while it illustrates the connection which different classes of truths have with each other, proves, that it is to a mind well fitted for the discovery and reception of truth in general that the evidences of religion are the most satisfactory."*

These considerations from this high authority we respectfully submit to the attention of Mr Lawrence. We believe him to be a man of talents and acquirements; but in the work now before us, he has wandered into speculations for which his intellectual habits are completely unqualified, and has "reasoned downwards." We fondly hope that he was not himself aware of the abyss into which his speculations were leading him. We think too highly of his understanding to believe that he is really convinced by his own reasoning; we hope too well of his heart to imagine that he seriously intended to lead his pupils into a system, dark as the valley of the shadow of death, and pestilential as the vapours of Acheron.

IV.

Observations on the Prevalence of Fever in various parts of the United Kingdom; and on the eminent utility of Houses of Recovery: exhibiting the great advantages that would result from such an Institution for the reception of the Sick Poor of Bristol and Clifton. By D. J. H. Dickson, M. D. F. R. S Ed. & L. S., Fellow of the Royal College of Physicians of Edinburgh, Physician of the Fleet, One of the Physicians to the Clifton Dispensary, &c. 8vo, pp. 34. Bristol, Barry and Son, 1819.

We need not pass any eulogium on Dr Dickson, as our former numbers have, from time to time, afforded the most satisfactory proofs of his superior abilities, both as an accurate observer and a skilful writer. We shall, therefore, pass at once to the subject-matter of his present tract, which may be regarded as a valuable synopsis of all that has recently been written on epidemic fever, and of the most practicable schemes for restraining and extinguishing it. We are the more anxious to lay before our readers an outline of these "Observations," as we fear their publication in a provincial town may deprive them of that extensive circulation to which their high merit justly entitles them.

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* Outlines of Moral Philosophy, p. 235, (4th edit.)

If every class of the profession, and indeed of the community at large, have not by this time obtained right notions of fever, both in its nature and treatment, we may well despair of that desirable event ever taking place; for almost every large town in the kingdom, from the one end to the other, can boast of treatises on the subject, published by different resident practitioners; and even the periodical press has cheerfully postponed more taking subjects, to inculcate homely directions for eradication or prevention. In this active career of benevolence, we trust we ourselves have not been behind hand. We have done our utmost to collect, in our late numbers, all the information acquired, whether from our personal observation or the correspondence of our contributors; and, in the present, we mean to devote a few more pages to the subject. To be sure, the epidemic is now happily on the decline, at least it is so in most places; but this circumstance will not supersede or destroy the interest of recent publications on the disease. These, on the contrary, will long command attention, and produce benefit; for we are willing to hope, that the present melancholy epoch has produced a degree of knowledge unequalled both in point of extent and accuracy;—knowledge which will be stored up against the time of need, and prove eminently serviceable in any future visitations of a like nature.

Dr Dickson's leading object is to prove the necessity of establishing a Fever Hospital or House of Recovery in the city of Bristol. His arguments for this purpose are extremely cogent; and, as they apply equally to other towns, we shall transcribe some of them.

> "The example of other places where the epidemic has been, and still continues to be prevalent to an alarming extent, and the certainty that its power in such places might have been greatly circumscribed, at least, if precautionary measures had been timeously enforced, speak in a language that cannot be misunderstood, and will deprive us, I fear, of even the satisfaction of a plausible apology if we should neglect their admonitions. What would be said of the conduct of an individual who, with the example of his neighbours around him, involved in one common calamity, should plead, as an excuse for neglecting those precautions which would insure safety to his own family, that they were yet untouched, and, therefore, that such precautions were uncalled for? If a ludicrous comparison may be pardoned on so serious a subject, he would stand somewhat in the predicament of Abel Handy, who, after enumerating his notable inventions in case of fire, from having none of them in readiness when the

castle was in flames, was driven to the extremity of hoping that it might 'go out of itself.'" P. 13.

An opinion is entertained by many members of the profession that cleanliness and thorough ventilation *alone* will succeed in preventing the propagation of contagious fever. This certainly will be found correct in upper life, or where the disease breaks out in the houses of persons in comfortable circumstances, who have the means of separating themselves, in a certain degree, from the sick. But the case is mightily different in the hovels or obscure lodging-houses of the poor, where a large family is often crowded into one dirty low apartment of a few feet square; and where, by consequence, the healthy must respire an atmosphere tainted with the exhalations of the infected. In such circumstances, we conceive, the disease must spread in spite of the most perfect ventilation that can be practised. This particular is well illustrated by Dr D.

> "Of the difficulty of disarming typhus fever of its infectious power, without adequate means of prevention and separation, I am entitled to speak from experience; for of eleven British medical officers, placed under my directions, to attend the sickly division of the Russian fleet in the winter of 1812-13, nine, including myself, were attacked with fever in the course of a few weeks. Two of this number died of the disease, and two of its consequences; I was one of the severest sufferers who recovered."
>
> "That the most scrupulous attention to cleanliness and ventilation cannot protect others exposed to an atmosphere tainted by a compact mass of human beings labouring under fever, I can also prove. In two English hospital-ships, which, from the emergency of the case, were excessively crowded, having received during the period of my superintendence above eighteen hundred patients; ventilation and cleanliness, though carried to the utmost extent, could not afford security from the want of sufficient space for separation. In the one hospital-ship which received the first, and, consequently, the worst cases that could be selected, not only the surgeon and other medical officers suffered severely, but twelve out of sixteen attendants, accustomed to the duty of waiting upon the sick, were seized with fever, four of whom died. In the other hospital-ship, several attendants, two assistant-surgeons, and ultimately the surgeon, were also attacked; all of whom, with one exception, recovered. These examples show how much more dangerous and inevitable is the fever produced by concentrated contagion." Pp. 20. 21.

These passages incontrovertibly prove the necessity of fever hospitals, and the following one places their great utility in a new and perhaps more striking point of view.

"It requires no stretch of foresight to perceive, that, provided fever does continue to prevail epidemically, the plan of a large fever hospital, or of smaller ones in different parts, must be adopted in all large towns and cities; and thus much misery, hazard, and ultimate expence may be incurred, which earlier measures might have prevented. But even if fever does not prevail, this measure is still highly expedient in such places. To employ a seeming paradox, a fever-hospital should be erected that it may *not* be wanted; for however great are its advantages in a curative, they are outstripped by its advantages in a preventive point of view. It is *the protecting influence which they extend to all,* which renders houses of recovery so eminently useful to society, and interesting to the legislator and philanthropist." P. 23.

We have long regarded as singularly injudicious that article in the rules of almost all hospitals and infirmaries throughout the kingdom, which peremptorily forbids the admission of patients labouring under fever, or other infectious diseases. When we reflect that such complaints form a large portion of the maladies of the poor, we can readily conceive how much distress is caused by shutting against the indigent those very asylums expressly established for their relief and comfort. We agree with Dr Dickson that this matter well deserves to be taken up by some benevolent member of the House of Commons. It should be imperative upon the governors of all public hospitals to set apart one or more wards for the reception of fevers and contagious diseases generally, which endanger the health of the community by their tendency to spread. It is surely contrary to the spirit of *justice,*—to say nothing of *charity,*—that misfortune should be treated like crime, and that a refuge should be refused to objects afflicted with fever, while it is granted to others labouring under complaints perhaps not of aggravated suffering at the present, nor of ultimate danger in the future!

Dr Dickson does not go systematically into the causes of the epidemic, but he lays great stress on that "atmospheric influence favourable to the production of fevers" which has reigned in our climate for the last three years, and which, we think, ought unquestionably to be regarded as the *causa sine qua non,*—the great auxiliary agent in the spreading and duration of the late sickness. The belief in "atmospheric influence," or certain "constitutions of the air," as productive of this or that order of diseases, is as old as Sydenham, and even older. In what those constitutions consist we are still, and probably ever will be, ignorant. But although we cannot satisfactorily trace the efficient cause, we are not the less certain, from the effects, that it really exists. When once we have ascertained the cause of those variations that influence the movements of the barometer, it will be soon enough to investigate those still more latent aërial conditions that affect human health. In the mean time, any speculations on the subject would at best be exceedingly gratuitous, and exemplify that " mera palpatio" of which Bacon has justly spoken with contempt.

At p. 28 the author declares that he has " but little faith in the efficacy of fumigations with the mineral acids," and recommends, in preference, ventilation, washing, and other modes of purification. We shall not stop to inquire whether this is not somewhat at variance with the passage (already quoted) where he so ably points out the insufficiency of ventilation and cleansing, when unaided by other means, to suppress fever,—but pass on to the general question about fumigation. Some living authors, at the head of whom is Dr Trotter, have contended, that since we know not the chemical qualities of contagion, it is unphilosophical, in the highest degree, to apply chemical agents for its extermination. But in reply to this we would remark, that, although we know not the precise properties of contagion, we are at least certain that it is an animal product. Therefore, the strong acid vapours, which are ascertained by uniform experience to act upon and destroy all other animal matters, whether solid, liquid, or gaseous, surely cannot be considered as misapplied when directed against contagion. Granting, however, for argument's sake, that contagion is an invisible and imponderable substance, quite beyond the sphere of the usual chemical affinities, still it evidently adheres to animal effluvia as its *nidus:* whether, then, the acid fumes attack and destroy the corpuscles of the contagious virus itself, or merely the *vehicle* by which the virus is conveyed, the effect is the same; the security achieved is as great on the one supposition as the other.

It must not be understood that what we have just advanced in favour of fumigation is meant to hold up that expedient as superseding ventilation, whitewashing, and accurate attention to cleanliness. On the contrary, the latter means are always of paramount importance, and should never be neglected. In fact it will be useful, in addition to these, to apply a very high temperature by means of stoves, for the extermination of contagion. This has latterly been done in Dublin; and it is particularly applicable to matresses and such other articles of furniture as cannot without injury be either washed or fumigated. All we mean to say is, that, in the dirty and confined alleys of large cities, *perfect* ventilation is often impracticable; and that, under such circumstances, acid fumigations form a valuable substitute. *

While on the subject of securities against infectious effluvia, we may mention one which has lately been recommended to the notice of the scientific world, (through the medium of Dr Thomson's Annals of Philosophy,) by Mr J. M. Bartlett. This gentleman advises persons exposed to contagion to use gauze veils as a preventive. We shall transcribe his letter on the subject, for the information of such of our readers as may not have access to it where it now stands. †

"Permit me, through the pages of your Annals, to suggest to your medical readers, and those employed in hospitals and other infected places, the practicability of using *gauze veils* as a preventive of contagion. This method was successfully employed by M. de Saussure and his party when he ascended Mont Blanc, to preserve their faces from excoriation; nor was their sight at all impaired, as is usually the case with travellers in elevated regions. When, therefore, we perceive the efficacy of a contrivance so simple in the rarest of mediums, and descend to the greatest depths of the earth to which the labour or ingenuity of man has penetrated, and find the same means made use of (viz. *gauze wire*) to prevent the combustion of the inflammable gases which abound there: why, let me ask, may it not be employed to the end proposed? It is an ascertained fact, that the miasmata which stagnant waters exhale is diverted by the intervention of a few shrubs only. (Of this the Pontine marches near Rome afford indubitable evidence, since whole families have resided near the spot for years, without having suffered from the mephitic vapours which those putrid waters engender; and for which no other cause can be assigned, than that a series of *trees* separates their abodes from those pestilential wastes.) It is also well known, that travellers are preserved from the suffocating heat of the *Siroc* of the desert, by merely bringing their faces in contact with the surrounding sand, the minute particles of which, in all probability, prevent the vapour from penetrating to the respiratory organs. Thus we find that all media exhibit the same phenomena, when opposed by the same difficulties; and, as far as reasoning from analogy will admit an inference, I cannot help subscribing to the belief of the practicability of what I propose. I beg leave, however, to submit it with great deference to your readers, sincerely hoping, that, should they deem it worthy of experiment, the result will be successful; since it would tend so materially to the advancement of the happiness of mankind. I have the honour to be, &c. &c."

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* The muriatic, though a weaker acid than the nitric, has a stronger affinity for animal matter; and therefore should be preferred in fumigating.

† Thomson's Annals of Philosophy, Vol. XIII. p. 833.

We are disposed to place very little reliance on the expedient here suggested, as it rests on grounds so purely hypothetical. Mr Bartlett draws his inference, as to the efficacy of his preventive, from analogies exceedingly loose and inconclusive; not to apply a harsher term. What person but himself will discover any resemblance betwixt the wire-gauze in the safety-lamp of Sir Humphrey Davy, with a view to restrain flame, and a veil drawn over the human face, to restrain "the winds of heaven from visiting it too roughly?" Every one knows that the safety-lamp acts upon a principle totally different; namely, that the burning gas, when it comes in contact with the wire-gauze, is immediately cooled down to a temperature no longer fit to support its ignition; and thus the flame cannot pass through the apertures of the wire to inflame the fire-damp, diffused at large through the pit. Mr B. is so fond of remote analogies, that we wonder he did not hit upon the fact of gauze thrown over fruit-trees, being a protection to the tender blossoms from the hoar-frost of the mornings and evenings. The analogy, in this instance, is somewhat more plausible, and seems, at first sight, to reflect a few rays of probability on his hypothesis.

Notwithstanding our declared want of faith in its efficacy, we are desirous that Mr Bartlett's suggestion should be tried: more particularly as, in a subsequent letter, (Annals of Philosophy for July 1819, No. 79, p. 71,) he states a case in which the use of a veil apparently prevented infection. Experience is the best test of truth: to the result of experience, therefore, we shall leave the expedient in question.

After this digression, it is high time that we should return to Dr Dickson. But our limits prevent us from discussing at further length the contents of his interesting tract. We shall therefore conclude with simply recommending it to the reader. The style is extremely chaste and good. As he himself assures us, he has neither aimed at eloquence nor pathos, but has addressed himself to the understanding, by arguments which require no subsidiary aid from the feelings or prejudices of any class of readers.

Upon the whole, we cannot close his tract without expressing a wish that Dr D. may soon present himself again before the public in some work of greater magnitude, if not of greater importance.

PART III.

MEDICAL INTELLIGENCE.

On Monday, August 2d, the Senatus Academicus of the University of Edinburgh conferred the degree of Doctor in Medicine on the following gentlemen, (117 in number,) after having gone through the appointed examinations, and publicly defended their inaugural dissertations:

Of Scotland.

A. D. Anderson, De Pneumonia.
James Anderson, — Hydrope Anasarca.
George Ballingall, — Apoplexia Sanguinea.
Benjamin Burt, — Rabie Canina.
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From Berbice.

Samuel Tyndall, De Rebus Lucem in Tenebus emittentibus.

From Santa Cruz.

Richard Tuite, De Cynanche Tracheali.

From the Isle of Wight.

T. Williams, ... De Pneumonia.

From Wales.

G. H. Jones, ... De Usu Aquæ et Methodis eam ad Mare Servandi.

Medical Lectures in the University of Edinburgh.

Dietetics, Materia Medica, and Pharmacy	Wednesday, Oct. 27.	8	Dr Home.
Practice of Physic		9	Dr Gregory.
Chemistry & Chemical Pharmacy		10	Dr Hope.
Theory of Physic		11	Dr Duncan, sen. & jun.
Anatomy and Pathology	Wed. Oct. 27. 1 o'clock.		Dr Monro.
Principles & Practice of Surgery	Wed. Nov. 10. 4 o'clock.		
Theory & Practice of Midwifery	Tues. Nov. 9. 3 o'clock.		Dr Hamilton.
Clinical Medicine	Tues. Nov. 9. 4 o'clock.		Dr Home. Dr Duncan, jun.
——— Surgery	Mond. Nov. 1. 5 o'clock.		Mr Russell.
Military Surgery	Sat. Nov. 13. 1 o'clock.		Dr Thomson.

Practical Anatomy under the superintendence of Dr Monro.

During the Summer Session, Lectures will be given on the following Branches of Education:

Botany by Dr Rutherford.
Midwifery by Dr Hamilton.
Clinical Lectures on Medicine.
Clinical Lectures on Surgery, by Mr Russell.

On Diarrhœa, as it appears on Shipboard, in the Polar Seas, by William Macartney Ross, Surgeon, Leith.

It has been remarked by Dr Lind, that individuals passing from a colder into a warmer climate, and *from a warmer into a colder*, are liable to be spontaneously affected with diarrhœa. And, in a proof of the latter proposition, he has noticed, that if the stationed ships from the West Indies, which are generally relieved at the expiration of three years, arrive upon the coast of England in the winter, and by contrary winds are kept long exposed to the cold, the men, having been accustomed to a hot climate, will be attacked with diarrhœas. It must often have been observed that sudden changes of temperature, particularly the great decrease which takes place in this respect after the autumnal equinox, are frequently the cause of this complaint. In the outward bound passage of the vessels employed in the whale-fishery on the coast of Spitzbergen, I have more than once had occasion to remark the very great effect of the transition into a cold latitude in deranging the state of the alvine discharge. The vessels destined for this often perilous voyage generally leave England about the end of March, when the weather is comparatively temperate. A week or two serves to convey them within the arctic circle, in the course of which time few cases are beginning to appear. But after being fairly within the limits of the frozen sea, and encompassed with ice, so that the wind even carries with it a strong and penetrating frost, the cases daily increase both in number and severity. The weather, becoming progressively milder after the beginning of May, and the seamen by this time being more inured to the climate, few or no cases are met with; and such as do occur I have always found to arise from the patient having been called suddenly from bed, in the course of duty, and exposed to an intensely freezing atmosphere. Those who have never been in the northern climate before are more liable to be attacked.

From the frequency of the stools, the griping, flatulence, and pungent sensation felt in the rectum, the disorder appears to be of that nature which Cullen terms *diarrhœa crapulosa*. The stools are of a watery nature, sometimes frothy, and not unmixed with blood; but the latter only occurs where the disorder has been of some continuance. The countenance presents a cadaverous and anxious appearance; the patient is thirsty, and the skin cold and rigid. The pulse is languid. The disorder is of a longer or shorter continuance as the patient is careful to seclude himself from the exciting cause, a circumstance attended with some difficulty on shipboard. The emaciation which succeeds might in some instances prove fatal, were it not for the salutary nature of the provisions used in this service; the beef from its solid nature, and not yet much taken with the salt, contributing much to a recovery, while the patient restrains himself as much as possible from fluids.

As the morbid cause of diarrhœa, in this instance, must be attributed to the suppression of the insensible perspiration, the employment of the antimon. tart., as a diaphoretic, in small and frequent doses, has been of essential service. I generally directed semicupium; and, in order to suspend the inordinate action of the instestinal canal, the employment of opium with cordial astringents was effective I, however, generally premised a slight aperient of magnesia and rhubarb with ginger, in order to remove any feculent matter which might be lodging in the intestines. The impaired tone of the parts was restored by the use of cinchona and a nutritious diet.

In the homeward passage the temperature of the atmosphere being considerably ameliorated, and the winds less calculated to convey the vessels quickly into a warmer climate cases of diarrhœa seldom appear unless the passage has been uncommonly speedy. In such a case, however, the inverse proposition of Dr Lind is confirmed.

I have alluded to the suppression of the insensible perspiration as the morbid cause of diarrhœa crapulosa, as it appears on shipboard in the Polar Seas; and this, in so far as the suppression of so considerable an evacuation must be compensated by the augmented discharge of the other, is satisfactory enough. But I am of opinion, that this alvine evacuation, if not frequently caused, is at least often considerably promoted from another quarter. The discharges of mucus and other humours secreted in the nose, fauces, palate, &c. become exceedingly copious from the intensity of the cold, and, being swallowed, appear to operate as a powerful cathartic. "Omnis saliva," says Boerhaave, "mucus, aliique secreti humores ex naribus, faucibus, palato, membranâ Schneiderianâ, ut quandóque catharsin moveant, ut in diarrhœa catarrhali quo in casu ad libros aliquot intra 24 horas nonunquam fluat." * It is observable, that this eminent writer's expressions are qualified by "quandoque ut in diarrhœa catarrhali," arguing, that these secretions do not always possess cathartic properties, and, probably, act only in this manner from the increased quantity of the discharge, when thus induced by extreme cold.

There is reason also to believe, that the atmospheric air itself, being exceedingly condensed, has some influence in contributing to the disease. The learned author above cited classes this fluid among the purgatives, "quæ deglutiuntur," after having first mingled itself with the saliva and the humours secreted in the œsophagus and palate. The flatulence accompanying the complaint is, at least, indicative of its presence in the alimentary canal.

In the preceding observations relating to the occurrence of this disease, I am corroborated by some who have had similar opportunities of remark.

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